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Editorial Notes

ANTIQUITY started as the private venture of a particular person, but he would never have started it if he had not felt that others beside himself needed an organ to express their point of view and to publish the cream of their researches. ANTIQUITY thus became, quite naturally and informally, the organ of the then younger generation, consisting of alert-minded students who were creating new implements of research and using them to give substance and form to prehistory. That phase is now nearly over in Britain, but in some other countries it has hardly begun. A few years ago one might have said that it had not begun; recently however a few European archaeologists, working independently, have begun to raise the standard of research to a higher level. This journal would like to call attention to their work from time to time.



The archaeology of a country may be judged both by the way in which its excavations are conducted and published, and by its museums. The writer of these Notes has recently seen something of the museums of France, Italy and Greece, and the impression left upon him is an unfavourable one. Take for example the Louvre, one of the most famous of all national museums. The contents are artistically fine and are often well displayed; at least one does not, as in some of the Breton museums, require a torch to see the objects. But the labelling is deplorable; the Etruscan Room, for instance, has none at all, except that a case containing Greek, Italian and Roman weapons is labelled 'Armes grecques, italiotes et romaines'. No provenance, no dates, nothing to help the visitor. In the Egyptian Department is a room with four cases containing more than a hundred objects all unlabelled. Some wooden objects are assigned to 'the 1st Theban Epoch', a description that conveys nothing to anyone but an Egyptologist. At the entrance to the rooms on the 1st floor is a painted iron plaque setting out the periods of Egyptian history, and the rooms assigned to them; the 'Theban Epoch' is not mentioned. To these examples, chosen more or less at random, could be added many others not only from the Louvre but also from many other famous European museums.



This absence of labels is a fundamental defect because an object without provenance or date is of little archaeological value even when it may be pleasing as a work of art. Yet a large proportion of the contents of many famous museums are unlabelled, nor do those responsible for this neglect of their duty seem conscious of sin. The Vatican Treasury contains priceless exhibits but almost nothing to tell the visitor what they are. The Prehistoric Museum in Rome is dustier and shabbier but otherwise in much the same state as when the writer made notes there in 1909. The Keramikos Museum at Athens

does not contain a single label. It is no excuse to plead that only specialists need labels, for when a specialist does visit the museum the curator may be away or he may forget to bring the key (this has happened). That kind of thing occurs regularly in Italy where locked cases and lost keys are quite common. Everyone knows this and it is a frequent subject of sardonic comment when archaeologists meet and exchange experiences ; but the facts are not often mentioned in print for fear of retaliation. Now that a few individuals are trying to improve things it is time to speak up and show that they have the support of their colleagues elsewhere. The present writer has suffered in silence for nearly half a century.



There was a time when hard things could have been said of many British museums, but not now. Some of the best museum technique is to be seen in our Wessex museums; here, as in the excavation which preceded, General Pitt-Rivers long ago showed the way in his museum at Farnham in Cranbourne Chase, Dorset. There the chief feature is the models of his excavations, and it is these which provide a clue to some of the shortcomings just criticized, in so far as these are not due to plain idleness and incompetence. We think some defects are the result of an unnatural separation from field-work, especially from properly conducted excavations. Where these are carried out and there is a good liaison, there you will be likely to find adequate labelling, supplemented by photographs of sites, plans and models. The National Museum at Leiden in Holland is a good example of this liaison, and of course there are many others.



It must be remembered that modern archaeology is a much newer thing than many museums, for most of them were founded in the days when excavation consisted mainly of grave-robbing and treasure-hunting. Some allowance must be made for this legacy from the past, for which the present curators cannot be held responsible ; many objects reached the museums from dealers and collectors, without history or provenance. Museums thus afflicted can only be cured by a major operation called the Clean Sweep. Examples of the Clean Sweep and its good effects may be seen in the Salisbury and Nicosia Museums.



It follows as a natural corollary that the best and liveliest museums are to be found where modern methods of research, especially of excavation, prevail. Much could, and should, be done to improve labelling, even where field-work lags behind ; but one can hardly expect to find a good, well-arranged group of exhibits where there have been no properly conducted excavations. A beginning is being made in France, Belgium and Italy, but it will be some time yet before the results can become apparent. A rich harvest is awaiting those who bring modern techniques to the excavation of sites, especially inhabited sites, in Mediterranean lands. There the stage of wholesale clearance is being succeeded by one of carefully conducted digging planned to achieve a limited objective. (Perhaps we should say 'supplemented' rather than 'succeeded', because the *complete* excavation of a site is still a most necessary thing). Professor Bernabo Brea, Director of Antiquities in Sicily, has already led the way at Arène Candide and more recently at the citadel of Lipari, where excavations (still in progress) have revealed stone houses of the Early Bronze Age with Mycenaean pottery imported from Greece or the Aegean. The

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potsherds are associated with local wares, so that we have at last a firmly fixed dating-point for prehistoric material in the central and western Mediterranean. Hard on the heels of this achievement comes the subdivision of Maltese prehistory into distinct periods, made possible by two things—the existence of stratified deposits, and the scientific excavation thereof. The famous megalithic monuments of the Maltese islands can now be seen to have developed from a primitive trefoil plan into one more elaborate. In their latest form they are regarded as temples; but ‘the earliest in the series are morphologically far closer to megalithic tombs than the later ones’. Many readers will have heard about these excavations from the broadcasts of Dr Ward-Perkins and Mr John D. Evans, published in the *Listener* (3 June and 22 July), and from that of Professor Piggott (delivered on 25 July).



While these excavations were being carried out in Malta others of the same precise kind, directed at a clearly envisaged target, were being made at Stonehenge, a contemporary megalithic temple. Each set of excavations is typical of the way we attack a problem to-day. Both at Stonehenge and at Malta there had been previous digging, but it had left many problems unsolved. Worse than that, it had inevitably destroyed evidence. At Stonehenge by a wise provision one half of the area of the circle had been deliberately left undug, so that future excavators whose technique was more advanced might have scope for applying it. In Malta no such provision was made, but fortunately not every cubic foot was dug up; and for this intensive digging quite a small block of undisturbed stratification is enough, thanks to the abundance of potsherds. (Professor Blegen was faced by exactly the same problem at Troy, and solved it by similar methods.) The museums at Farnham and Salisbury reflect this careful scientific excavation; there is in fact a direct link between the excavations of General Pitt-Rivers and those now in progress at Stonehenge, for all Wessex diggers are proud to acknowledge their debt to the great pioneer.



The excavations at Malta have already produced important results of a more theoretical kind. ‘We can state with fair confidence’, says Mr Evans, ‘that the temples themselves cover a period of about 500 years, from just before 2000 to about 1500 B.C.’ The earliest pottery in Malta (from the cave of Dalam) ‘is identical with that of the earliest neolithic inhabitants of Sicily, and suggests that the Maltese islands were first colonized from there by primitive farmers late in the third millennium B.C.’ This is the first time, we think, that the origin of food-production in the western Mediterranean has ever been mentioned. We do not remember to have read any discussion of the route by which food-production spread westwards across the Mediterranean. How did the knowledge of agriculture (and the necessary seed) first reach Italy, Sicily, Malta, northwest Africa and Spain? What, for instance, was happening in Algeria and Tunisia during the Late Neolithic and Bronze Ages? Is the gap in our knowledge real and due to absence of evidence, or is it merely the result of French concentration on flints? We ask these rhetorical questions in the hope of calling attention to a neglected line of research. So far as northwest Africa is concerned we would suggest that a start might be made by a survey on modern lines of the megalithic burial-chambers and rock-cut tombs near Constantine, looted during the 19th century.

The British Museum and British Antiquities*

by SIR THOMAS KENDRICK

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IN the year 1849 a Trustee of the British Museum, Mr W. R. Hamilton, at that time a man of 73 noted for his great interest in archaeology, particularly that of Greece and Egypt, was giving evidence before a commission enquiring into the affairs of the Museum. 'Have you', he was asked, 'ever turned your attention to the question of extending and improving the collection of British antiquities in the British Museum as distinct from all others?' 'I have not', answered Mr Hamilton.

The same question was put to a much younger Trustee, Viscount Mahon, who was President of the Society of Antiquaries and already an historian of some note. By his subsequent name, Lord Stanhope, he is well known to us as the founder of the National Portrait Gallery. 'Have you', he was asked, 'turned your attention at all to the question of the establishment of a separate Department of British Antiquities in the Museum?' 'No', said Lord Mahon.

In this paper I propose to describe how the British Museum changed its mind on the subject of British antiquities, and to suggest that it was largely due to the British Museum collections, and the intelligent appreciation of them by a great British Museum curator, that there came about a general revaluation of the status in civilization of the ancient Britons. I shall then ask whether at this period a hundred years ago it would have been better not to have entrusted the serious care of British antiquities to the British Museum, but instead to have formed a separate Museum of National Antiquities, though I must add that this is a question to which I do not intend to give you a complete answer.

The Commissioners had good reasons for putting the question that they addressed to these Trustees. They were aware that a small group of antiquaries in this country was becoming very indignant about the way in which the British Museum was neglecting national antiquities. They had received a letter from John Yonge Akerman submitting resolutions of the British Archaeological Association on this subject. They learnt that five years previously Lord Prudhoe, afterwards Duke of Northumberland, had offered the Trustees a most remarkable collection of ornamental bronzes (British, Roman, or Saxon, no one really knew which) that had been discovered at Stanwick, 'provided that a room were appointed in the Museum for the reception of national antiquities'. They learnt, of course, that the British Museum had acquired the Stanwick bronzes, but had not yet provided the room. They discovered that even after this enrichment, the British antiquities in the Museum formed 'little more than a basis' for the creation of a systematic collection of the kind desired by the outside complainants. They concluded with a message to the Treasury—namely, that the formation of a connected series of relics illustrating the arts and manners of the various races which have occupied our soil is an object 'to which the liberality of Government might be directed with unquestionable advantage!'

Actually, by 1850, the British Museum had taken *some* action in this matter. As long ago as 1838 it had been decided that Romano-British stone monuments should be

* A paper read at the Museums Association Conference at Belfast on 19 July 1951, with Dr D. B. Harden in the chair. Reprinted from *The Museums Journal*, Vol. LI, no. 6, pp. 139-49, September 1951.

arranged in a gallery off the entrance hall, so that the collections of marbles could be viewed in their natural order (if you first walked through with your eyes shut and did your sight-seeing on the return journey) Egyptian, Etruscan, Greek, Roman, and British—and that on the first floor there should be eventually a series of rooms ‘for small Greek, Roman, British, and miscellaneous antiquities’. In 1845 the promise to provide two rooms for the British collections had been made to Lord Prudhoe in return for the Stanwick bronzes. In 1848 the Romano-British monuments had indeed been assembled in what is now the Roman Gallery, and a new room on the upper floor, not yet completed, had been allotted to the smaller British antiquities. On the 29 November 1850 comes the satisfactory news that ‘the (upstairs) British Room will be ready for use about Christmas’.

The completion of these two rooms was no more than the critics of the British Museum deserved as a recognition of their just complaints. They were an energetic set of people, deeply interested in their national antiquities, and in the preceding five or six years they had done a great deal to promote the archaeological study of the British past. It may be added that in doing so they had stirred up the largest and angriest row in the history of antiquarian societies in this country, a row still commemorated by the grouping of their descendants into the two bodies into which in 1845 they exploded themselves, the British Archaeological Association and the Royal Archaeological Institute of Great Britain and Ireland. Their first notable achievement had been a congress at Canterbury in 1844, which they themselves described as ‘one of the most extraordinary movements of this age of progression’. ‘The antiquary was no longer an object of ridicule’, they told us—though they ran, one would have thought, not a little risk in that respect in as much as one of their ways of entertaining Canterbury was the public unrolling of an Egyptian mummy. But these congressists were in general workmanlike and serious, with their section of ‘Primeval Antiquities’ that extended to the middle of the 7th century A.D., the Medieval Section, the Architectural Section, and the Historical Section. Their subsequent meetings and their two journals, and the books written by their members, witness to their enthusiasm and capacity for work. I will mention only Akerman’s *Archaeological Index* of 1847, and a book published two years earlier, Thomas Wright’s *Archaeological Album, or Museum of National Antiquities*.

We must recognize that these good people really were museum-minded and played an important part in the history of our museums, for it was the custom of these antiquaries to form temporary museums of national antiquities—generally with a regional interest—in the towns which they visited. Thus, in the gallery of the Deanery at Winchester in 1845, and at St. Peter’s School, York, in 1846, the Archaeological Institute was exhibiting to its members some first-class prehistoric antiquities, including the Stanwick bronzes, the collection that was simultaneously being used to spur the British Museum into activity. At Worcester in 1848 the British Archaeological Association formed a temporary museum of local antiquities in the Natural History Society’s rooms.

No wonder these ardent people felt that the British Museum was not giving satisfaction. At Winchester in 1845 the Secretary of the British Archaeological Association, Mr Pettigrew, expressed publicly the hope that a proper museum of natural antiquities would be formed. ‘The British Museum’, he said, ‘contains only particular specimens, not a series minutely illustrative of the antiquities of various nations and times, and it is specially defective in that which more particularly relates to us, and which should certainly characterize a national collection’. About the same time Mr Wright said in his *Archaeological Album* (p. 149): ‘In the British Museum our native antiquities appear to be held in very little esteem, and, in general, articles sent there are lost to public view. It is discreditable to the Government of this country that we have no museum of national

antiquities, which might, under a judicious curator, at a very moderate expense to the nation become one of the most interesting and popular institutions of the metropolis'.

Of course, the enthusiastic ladies and gentlemen of the British Archaeological Association and the Archaeological Institute were speaking for themselves alone. Ordinary educated opinion in England had not yet caught up with all this ardour for Britons and British antiquities. Most of the books that mentioned them were anything but enthusiastic. Gone was the magnificent swaggering Briton of Trojan descent, the follower of our famous Brutus, a prosperous, well-dressed man. Gone, too, was the dear romantic noble savage, who, likewise created in 16th century thought, had become especially popular in the 18th century. The early Briton of our period was an ignoble creature for whom one felt only a little pity and a great deal of contempt. The rot had set in in the second half of the 18th century. Our excellent poet William Hayley, in his *Essay on History* (1780), wrote :

Tho' Patriot Love the curious spirit fires
With thirst to hear th'achievements of his Sires ;
And British story wins the British mind
With all the charms that fond attention bind ;
Its early periods, barbarous and remote,
Please not, tho' drawn by Pens of noblest note ;
O'er those rude scenes Confusion's shadows dwell,
Beyond the power of Genius to dispell ;

Historians had not got much further than Edmund Burke's opinion (1757) of early Britain, namely that ' destitute of all those improvements which in a succession of ages it has received from ingenuity, from commerce, from riches and luxury, it then wore a very rough and savage appearance. The country forest or marsh ; the habitations cottages ; the cities hiding-places in woods ; the people naked or only covered with skins ; their sole employment pasturage and hunting '. Or that of David Hume (1761), who said of the pastoral Britons that as they were ' ignorant of all the refinements of life, their wants and their possessions were equally scanty and limited '. By 1849 Macaulay was prepared to say that the early Britons ' were little superior to the natives of the Sandwich Islands '.

They really were almost actively disliked. 'The people lived in holes in the ground', Mrs Markham told her three children, and she could not even impress Richard with what she considered to be the Britons' most remarkable achievement, the scythe-wheel chariot. ' I think they must have cut their own legs . . . sometimes ', said this intelligent boy. Little Arthur would have agreed with him. ' I am sorry to say that the old Britons had no churches, and they did not know anything about the true God ', he was told in 1835, and he saw at once that they were ancestors of whom one must be ashamed. He was given a very discouraging account of them, though he was informed that in some parts of the country the Britons used coarse earthen plates and bowls, and it was admitted that they were brave—which indeed they needed to be, for ' the poor Britons were almost naked, and had very bad swords and very weak spears '; in fact, ' the bad swords of the Britons could hardly ever hurt a Roman '. Little Arthur knew that this was a very good thing really, for ' when God allowed the Romans to come and take part of the country . . . He put it into the hearts of the Romans to teach the Britons most of the things they knew themselves ', and, ' what was better than all the rest, the Romans built some schools . . . and the little Britons were allowed to go to school as well as the little Romans, and . . . you may think how glad their fathers and mothers were to see them so improved '. For

Charles Dickens too, when he wrote *A Child's History of England*, published in book form in 1852, the Britons were only 'savage islanders'. Even in his own Kent, where they had come to some extent under foreign influence, they were 'almost savage'. It was true that they were brave and strong, and knew a lot about horses, and were clever at basket-work and building fortresses; but they were a poor lot all the same. They made no coins, and their bronze swords were awkwardly shaped, and so soft that a heavy blow bent them. I feel quite certain that when thoroughly nice children like Richard, George, and Mary Markham, and Little Arthur, and Dickens's young readers, were taken to the British Museum, they would never have dreamt of enquiring if there were a room set aside for the reception of British antiquities, though in all probability the precocious offspring of the members of the British Archaeological Association and the Archaeological Institute possessed in this respect a considerable museum-nuisance-value.

Not even the history-books for the grown-ups put up a case for the ancient Britons. The sources were collected and republished with translations by George Petrie in 1849 in his *Monumenta Historica Britannica*, but there was nothing new in this great corpus to startle historians into changing their minds. The Britons had Druids; Stonehenge was their temple; they were woad-painted; they were excessively quarrelsome; they had coracles and scythe-wheeled chariots; they lay buried, heathen-fashion, under tumuli; and the best that could be said of them was that those living in south-eastern England were probably less revolting than those who lived to the north and west of them. It made a poor tale, and attempts to popularize the urns and beads and bone implements and so on found in the tumuli—as in the four plates allotted to the Duke Collection at Amesbury in an 1816 volume of engravings entitled *Antiquarian and Topographical Remains . . . in Great Britain*—had met with little noticeable success. Generally, ordinary educated people did not like the Britons, and the best sort of thing that could be said about them at that time will be found in the 5th (1849) edition of John Lingard's *History of England*. You will also find much the same thing at the beginning of Francis Palgrave's *History of the Anglo-Saxons* (1831). The Britons did not come into dignified sensible history, and the archaeologists with their clay pots and beads were getting nowhere. It was their kind of stuff that had caused a bishop of the Church of England to describe the Historic Muse of his day as 'after much vain longing for a vigorous adorer, now fallen under that indisposition of her sex, so well known by a depraved appetite for trash and cinders'.

Having noted the enthusiasm for British archaeology of a small group of persons, and the indifference and disdain of the larger world of scholars, let us now go back to the British Museum. First of all, let us say frankly that the collections in the British sections were not of the kind that would obviously and instantly impress Little Arthur. Downstairs in the Roman Gallery were some interesting, but rather dreary, sarcophagi and stelae that were hopelessly eclipsed by the spectacular and much more important carvings that surrounded them. Upstairs there was a small series of minor antiquities on which, during the preceding twelve years, the Trustees had expended on an average about £30-£40 a year. There was already a respectable show of Bronze Age gold ornaments, mostly Irish, and a small quantity of stone and bronze implements, of which only a few actually came from England, and a fine series of bronzes of the Stanwick kind, including the Polden Hill finds, the Drummond Castle armlets from Scotland, and an Irish bronze altar-disc. No one as yet knew the real date of these beautiful but puzzling exhibits. There were also some poor oddments known to be of the Saxon period, a great hoard of smashed silver ornaments and silver ingots from the Viking Period hoard at Cuerdale, Lancashire, and a fine Viking Period sword from the River Witham. The most impressive antiquity from Roman Britain, the head of the great bronze statue of Hadrian that had stood in Roman

London, is not listed as a British antiquity in the guides of the period, and I suspect that it lived with other Roman bronzes in the new Bronze Room.

That is the first point. The British collections did not amount to much. It is also necessary that you should realize that the British Museum was at this time preoccupied with other and grander matters. The building itself must have seemed to its staff brand new, for Montagu House had only recently disappeared. The twelve years before 1850 had, indeed, witnessed the most remarkable changes in our buildings. In 1838 the old house, with its Towneley Gallery extension reaching northwards, still stood, forming the south face and the south-western portion of a great new quadrangle, the rest of which was the King's Library (1823-6) as the east wing, a north wing, and half the west wing, to which had been added a projecting gallery that was the new Elgin Room (this is the gallery in which the east pediment sculptures of the Parthenon are now shown). Ten years later (1848), the old house had vanished entirely, and also the Towneley Gallery, and the present south front had been erected; the west wing had been completed, with an extension in the form of the Arch Room and the old Print Room, and a new Lycian Gallery had been built (now the Archaic Room).

The Front Hall was opened in 1847, and also the Main Staircase leading to the new galleries in the south-west corner of the upper floor where the smaller British antiquities were going to be housed. The excitement caused by new building continued in the next decade as well. By 1851 the gallery that is now the Ephesus Room had just been completed, and also the Nimrud and Nineveh Galleries. Eight years later the Second and Third Graeco-Roman Rooms were ready, and in the meantime (1854-7) Panizzi's celebrated Reading Room had been built in the spacious interior quadrangle.

To all this tremendous excitement over new buildings must be added the thrill of the most marvellous new acquisitions, particularly the newly-installed Lycian Marbles, all the more interesting because they were the subject of an important exhibition-policy dispute between Sir Charles Fellows on the one hand and the Trustees and Sir Richard Westmacott on the other. But the great wonder, a wonder that had really thrilled the country, was the arrival of the great Assyrian sculptures from the famous excavations at Nimrud and the nearby sites. Some of these had been placed in the Front Hall when they arrived in 1849, and in 1851 had been only recently installed in the Assyrian Transept. Everybody interested in archaeology was talking about them. You really could not expect anybody, apart from a few cranks, to take much notice of the poor British antiquities in the Roman Gallery and in the new room upstairs.

It is, therefore, understandable that the Trustees, deeply preoccupied with other and much more weighty matters, should still have had very little sympathy for British remains. Those in the Museum who were trying to establish them as something of importance must have felt that there was a disappointing lack of encouragement in the Board Room and in the office of the Principal Librarian that was hampering their work.

The most unfortunate incident was the deliberate refusal to buy the Faussett Collection in 1853-4. The Trustees were presented with an extraordinary opportunity. What was offered to them was mainly a series of Jutish antiquities from cemeteries excavated in the 18th century in East Kent, and though the nature and importance of the finds was imperfectly understood, the obvious richness of the jewelled gold and gilt brooches and pendants, the wealth of beads, glass and pottery, ought to have impressed any beholder with the great interest of the series, to say nothing of the fact that it included the superb Kingston Brooch. The Trustees were asked to buy the collection for £683, and they at once declined to give so large a sum. The Archaeological Institute immediately protested, and was told in reply that the Trustees had insufficient funds—which was true.

The Society of Antiquaries protested. Mr Wylie offered in vain his series of Anglo-Saxon finds from Fairford if the Trustees would buy the Kentish series. The Archaeological Institute petitioned them to make a special application for funds to the Chancellor of the Exchequer. The Trustees met again and once more summarily declined the offer. Parliament called for the papers, but the result was that the Trustees' decision was not disturbed. In the end the collection was bought by the generous and great-hearted antiquary Joseph Mayer, who presented it in 1867 with the rest of his private museum to the Liverpool Corporation, and it is now, as all British archaeologists know, a part of the collections of the City Museum. *Inventorium Sepulchrale*, published in 1856 and ably edited by the angry Charles Roach Smith and financed by Mr Mayer, is a permanent reproach to the British Museum.

The British Museum was at that time—say about 1850—divided into six Departments under the Principal Librarian—Printed Books, Manuscripts, Zoology, Geology, Botany, and Antiquities. The Keeper of Antiquities was a very remarkable and deeply learned man of 70, Edward Hawkins, best known as a numismatist, but also an authority on classical sculpture, papyri, and cuneiform inscriptions. Hawkins had been Keeper since 1826, and it was only two years after his appointment as Keeper that he had announced his intention to reserve a room in the new buildings exclusively for the exhibition of British antiquities. He probably did not pretend to any special knowledge of the collections that were going to be put into this gallery, but he had a genuine interest in the ancient Britons because they used coins. He was not a pioneer in this matter. Taylor Combe, his predecessor and the first Keeper of Antiquities, had prepared five plates of British coins as early as 1803 for a projected catalogue (they were subsequently used with unsatisfactory acknowledgment in Ruding's *Annals*), and Hawkins gave only two plates to them (engraved by Fairholt) in his famous *Silver Coins of England* that appeared in 1841, but he said that they were pre-Roman and British in origin, and saw that they gave a special interest to the ancient Britons, in as much as they were the beginning of the numismatic story of this country. Here then—in the Medal Room—was the beginning of intelligent curatorship of our British antiquities, but what Hawkins needed was a young man to take up the whole subject properly. His assistants were then (1850) Samuel Birch, orientalist and Egyptologist, who was 37; Charles Newton, who was 34, the classical archaeologist; and William Vaux, aged 32, an authority on ancient art, who was following Hawkins as a numismatist. And there was one vacancy. I should now like to record my opinion that the proper appreciation of British antiquities in the British Museum begins with the filling of this vacancy, for in 1851 Hawkins recruited to his tiny team one of the most remarkable men that the Trustees have ever had in their service, a young man of 25 called Augustus Wollaston Franks.

British antiquities did not have an easy time even after Sir Wollaston Franks had joined the staff, and in 1855 there was in fact another serious rebuff, for the Trustees at first refused to buy the great Roach Smith collection of antiquities from Roman London, which Hawkins asked them to acquire for £3,000. But here the Trustees were resisting because they thought the price was too high, and eventually they were successful in acquiring it for £2,000. The situation was tricky, owing to the influence of Roach Smith and his friends, who were already angry and disappointed with the Trustees, but the acquisition was safely made in 1856, and it was a turning-point in our history. Roach Smith had wanted the Corporation of London to buy his collection as the basis of a London Museum. Its transference to Bloomsbury was finally achieved because by this time the British Museum had proved itself to be the only possible home for it. The 1855 *Synopsis of the Contents* had described in a special section the Anglo-Roman antiquities,

and also the British and Medieval Room, and had declared itself as having views—not entirely friendly—about the ‘Stone and Bronze Periods of the Northern antiquaries’. The British past was now being taken seriously, and the visitor was actually invited to inspect ‘the rude half-baked pottery which is found in the barrows of the early Britons’. In the 1859 *Guide*, the authorities of the British Museum had heard also of the ‘Iron Period’, and were now calling attention to a series of antiquities, mostly of bronze, that bore a special kind of scroll-decoration, often enamelled, which were described as being ‘Late Celtic’, i.e. about the time of the Roman invasion of Britain. In Cases 36 and 37 the visitor was asked with justifiable pride to regard ‘a fine bronze shield found in the Thames near Battersea’. Truly there had been a notable change of mood from that of 1850 when the Trustees said they had never given a thought to the creation of a separate and properly cared-for British collection.

As far as the British Museum was concerned, this change was undoubtedly due to the influence of Franks, but the new view as expressed to the outside archaeological world was that of two men, Franks and John Mitchell Kemble. Their message, which would have seemed most unlikely and surprising to Mrs Markham’s still numerous readers, was that the early Britons of the immediately pre-Roman period were not contemptible savages, but craftsmen skilled in enamelling and metalwork, and possessors of a thrilling and unusual decorative style. ‘Late Celtic Art’ they called it, and the name became well-known.

In 1857 Franks was 31 and Kemble, who died in that year was 50. He was an exceptionally brilliant man, both in scholarship and social attainments, and he was closely connected with the British Museum, chiefly on account of his work on our Saxon manuscripts. Indeed, for a long period of his life he had lived in Great Russell Street, and he had lately returned to England after residence in Hanover, where he had been studying prehistoric archaeology. He and Franks had been associated in the preparation of the ‘Art Treasures Exhibition’ held in Manchester in 1857, and they had together decided to put Late Celtic Art before the world. In this Kemble has the priority, for his discourse to the Royal Irish Academy, in which he described and praised this ‘Britanno-Keltic art’, as he called it, was delivered in Dublin in February of that year, six weeks before his death; but Franks was simultaneously writing a corresponding statement in his article *Vitreous Art*, published in 1858 as part of the catalogue of the Manchester exhibition. To-day the best-known witness to this arresting claim on behalf of the ancient Britons is *Horae Ferales*, based on Kemble’s work in Germany and published with notes on the antiquities of the Late Celtic Period by Franks in 1863. Franks therein firmly established the real date of these antiquities—for it was still necessary to discuss whether they were Celtic, Roman, Saxon, or Danish—and in insisting that they were, as we should now say, ‘Early Iron Age’, Franks remarked:

‘The only reason, in fact, why antiquaries should hesitate to place these antiquities in a period previous to the *permanent* occupation of Britain . . . is the opinion prevalent as to the barbarous state of the inhabitants of this country’.

And then he goes on:

‘It is certainly in the National Museum of this country that a collection should be formed of remains so intimately connected with its early history’.

Actually ‘the National Museum of this country’ had been going through some serious strains and stresses. There was great controversy in progress on the subject of the suggested removal of the natural-history collections, and a lesser hullabaloo that affected British antiquities; for it was now argued by some that the British and Medieval

collections should be chopped in two at 'the epoch of Constantine the Great', and that only the British and Anglo-Roman collections should remain in the pagan darkness of Bloomsbury, the rest being transferred to a properly civilized museum of Christian art in South Kensington. The advice to retain the whole collection at Bloomsbury prevailed, largely as the result of a sensible statement of practical museum difficulties by Albert Way, the founder of the Archaeological Institute, given before a Select Committee in 1860, and our famous Panizzi, the Principal Librarian, who had favoured the split, had to content himself with a reorganization of the Antiquities Department on Hawkins's retirement in that same year. 'Greek and Roman', 'Egyptian and Oriental', and 'Coins and Medals', were obvious new Departments, but 'Ethnography, British Antiquities, and Medieval Antiquities' formed an awkward residue with no one to care for them except an assistant aged 34. The first idea was to join them to Greek and Roman antiquities, but they were in fact put under Samuel Birch, the first Keeper of the new Department of Egyptian and Oriental Antiquities, Franks being his assistant. It was not until 1866, when Franks was 40, that the Department of British and Medieval Antiquities and Ethnography was formed, with Franks as Keeper. Thereafter there was considerable progress. By 1887 there was a large 'Prehistoric Saloon' at the top of the main stairs with the Greenwell Collection in process of installation, and separate galleries for Romano-British, Anglo-Saxon, and Medieval antiquities. And at that point I shall leave this story, for the rest is fairly well known to you, and all I desired to do here is to tell you under what circumstances the British Museum decided to take care of British Antiquities in the present way that we *do* try to take care of them and accord them a reasonable prominence.

What I want to ask is whether we museum-curators are glad or sorry that there has never been a Museum of National Antiquities in London? It is a matter on which the English must not offer excuses. There was plainly an agitation in the mid-19th century that there should be such a museum, and Scotland showed us with what a wealth of exhibits and in what an interesting manner the job could be done. There the collection of national antiquities founded in 1780 by the Society of Antiquaries of Scotland was converted into a government-supported museum of national antiquities in 1858. By 1866 when Franks became Keeper of the newly formed Department of British and Medieval Antiquities and Ethnography, there were at least half-a-dozen important museums of national antiquities abroad all of a kind that—though the range of their contents do vary—might well have been imitated in England. In addition to the Scandinavian museums that provided, so to speak, the approved patterns in national museums, we may recall—as an example likely to have impressed museum opinion in England—the foundation of the Musée des Antiquités nationales at St. Germain-en-Laye in 1862. France, you will remember, had already the Cluny Museum, founded in 1844, and taking the place of the Empire Musée des Monuments Français.

Yet we did nothing about it, and as the years pass we have seen how successful many of these museums have been. I like them myself, and I know that they do provide a satisfactory, convenient, and agreeable introduction to a country's past (excluding, of course, recent history). The Museo Arqueologico Nacional in Madrid—which I mention because it happens to be the one of which I have latest experience—seems to me to be a quite admirable example. I do not think, however, I should begin to pick and choose among *real* national museums, so I will take as a target in this matter, in order to subject the British Museum to the cruellest comparison, an institution that we all know well, the ideal Museum of National Antiquities in Utopia—or let us call it Ruritania. There it is, a magnificent modern building with plenty of room for expansion and in fine grounds, the lawns in front being adorned with some rebuilt—and therefore useless—Ruritanian

megaliths, and some rude Ruritanian sculptures of the Dark Ages, easily digestible evidence of the fact that the Dark Ages in Ruritania had been dark indeed. You will remember the superb Entrance Hall with its colossal relief-map of Ruritania—that talks at you by gramophone and illustrates its own lectures by sparkling into changing arrangements of coloured lights ; the well stocked sales-counter, loaded with learned and weighty works as well as a gorgeous display of more popular material, and so sensationally staffed that we realize at once that in Ruritania directors prefer blondes ; the abundant signs in fluorescent lighting informing us how to reach the ice-cream parlour, the cinema, lecture-hall, rest-room, playroom, crèche, and chemist's shop, and the attractive restaurant, of which the staff, charmingly attired in Ruritanian native costumes, show that in this museum the director also collects brunettes. It is, however, only the archaeological collections that interest us—not always easy to find, owing to the superb showmanship with which they are presented, these being housed in a chain of galleries that are both beautiful and practical. Stone Age Ruritania with its dioramas and life-size dummies arranged in occupation-groups, the walls rough-cast in plaster imitating rock-shelters and covered with rock-paintings. The table-cases containing some real flint implements almost seem out of place, I think. Bronze Age Ruritania, the gallery celebrated for its frescoes by famous modern artists, contains among other attractions four Bronze Age trumpeters who blow their trumpets if you press a button, and the full-sized reconstruction of a manned and beflagged Bronze Age ship. And so on, and so on. The Room of Romano-Ruritanian Antiquities, surpassing all that has gone before, is artfully contrived to represent in successive stages the interiors of a forum, a temple, baths, a villa, and a fort. Mosaic pavements spread out on the floor ; the Roman fountain which spouts, though it is itself obviously bogus, undeniably genuine water into a little Roman garden, where is to be found one of the most fascinating exhibits in this great museum, a living colony of edible snails. It is all beyond praise, and continues to be splendid in all the rooms representing the succeeding periods. 'By Jove', my friends say to me as we progress, 'you must be green with envy'!

Why I am looking green is my own business. It happens that I should myself prefer the national antiquities of Ruritania to be housed in a dark, draughty, and entirely unsuitable old castle with plenty of spiral staircases. I like that sort of museum. But let me here pay a quite sincere tribute to these new buildings, to the learned and enthusiastic staff, and to the real care they have given to their collections, and let me go on to admit, as I must, that our British collections in London are not so handsomely or spaciouly shown. Let me admit, to put the matter in its worst light, that many visitors to the British Museum enjoy our collections without realizing that it contains any British antiquities at all. Yet though we acknowledge these and other shortcomings, I personally, believe that our British antiquities are better off where they are in the great Bloomsbury building than in a separate museum, however spacious and splendid. There are three points in favour of our system.

The first is only a small point, but it is not unimportant. If you house the national antiquities in a museum stuffed full of other nations' antiquities, as in the British Museum, your own national antiquities have the benefit of a range of scientific and technical services of a kind that can only be provided in a big museum with omnivorous interests. A museum of national antiquities in England would not have the money—you know this as well as I do—to maintain a research laboratory of the kind that we maintain in the British Museum ; and even if it had a laboratory at all its staff could not be expected to possess a tenth of the wisdom and experience of Dr Plenderleith and his colleagues. Thus it comes about that it is because the valuable and difficult group of Saxon antiquities

from Sutton Hoo came to the British Museum—and not to a smaller museum of national antiquities—that they have been better looked after than they would have been in any other museum in the world. I am sorry to appear boastful, but I do value the work done on Sutton Hoo so highly that I believe that in matters of study and conservation it could not anywhere have been surpassed.

The second point is that it is generally beneficial for one's national antiquities that, within the walls of a single building, they should be exhibited and studied side by side with the antiquities of other nations. I have had myself practical experience of this, and I know how valuable it is when one is working on British antiquities to have at one's call the knowledge, experience, and help of a team of learned colleagues trained in other archaeologies. My colleagues, Rupert Bruce-Mitford and John Brailsford, would agree with me that their published estimates of the significance of the tremendous finds, the Sutton Hoo ship-burial and the Mildenhall Treasure, are recent examples of the usefulness of having close at hand, available for daily and informal consultation, friends in other departments able to contribute indispensable knowledge derived from their own different provinces of research. No official travels for study-purposes can take the place of this comfortable counsel from immediate colleagues. I believe, though I cannot prove this directly, that the value of living in the same building with students of other archaeologies was appreciated in the very earliest days of our specialized study of British antiquities; and I think it was because the famous Three Age system of the northern antiquaries was believed, in a museum of catholic interests like ours, to be derived from a too rigidly confined study of Scandinavian antiquities that Franks and his friends were reluctant to adopt it as an inflexible system for Britain. In our 1855 *Synopsis of the Contents of the British Museum* we adopted the 'Stone and the Bronze Periods of the Northern Antiquaries' only as a basis of gallery-arrangement, but with the plainly stated warning that since stone implements were not discontinued entirely on the introduction of metal, objects of the same material were placed together 'without regard to which of these two periods they belong'. I think that, for 1855, that was a most perspicacious and creditable observation. Frank's friend, John Mitchell Kemble, expressed more fully in 1857 what the British Museum was here trying to say. In his Dublin address are these words: 'Let us, with the full spirit of an enlightened patriotism, devote ourselves to the illustration of our own antiquities; let us love them, and, loving them, labour to bring them to light; but let us not believe that they are all we have to learn, or that they convey all that ever can be taught. Let us look upon them only as links in one great chain, which embraces many nations, and many periods of human culture, which has no place of its own, unless considered in co-ordination with other links in a still greater chain . . . Let us be sure that we are not exclusive, but comprehensive in what we do; and let us, above all things, never lose sight of this great truth, that the interests of man have at all times led to a close communion between the several divisions of his race—that nothing can be dissociated in History, and that nothing must be dissociated in the study of Archaeology'. To those wise words I, and I think every British Museum man, would respond with a grateful and sincere Amen.

Kemble has brought us to the heart of the matter—to the third and most important thing. The great museum I represent is traditionally eclectic. We are derived from a jolly collection of universal curiosities. Sir Hans Sloane ranked a British antiquity as no more than an ordinary oddment to be set without special comment side by side with any other antiquity. In his catalogue a Bronze Age palstave from Kent is linked with an Indian cinerary urn from Jamaica, a Roman lamp from Cologne, and an Egyptian idol with hieroglyphs. And these antiquities had been collected in the same all-embracing

way as his stones and fossils and metals and shells and animals and plants. A nationalist element in collecting is not in our make-up, and I do not think that before the mid-19th century anybody in England had ever given any thought to the creation in a central place in the national interest of an ostentatiously British collection since John Leland in the 16th century tried to give a nationalist flavour to his gathering of books for the royal libraries. The British Museum in its origin and in its continued purpose takes a world survey, and concludes that British antiquities are, after all, only tiny tesserae in a colossal general mosaic. We do not exalt them unduly, and this is very good for British antiquities. I have seen, and some of you have seen, what lamentable mockeries of truth are only too easily achieved in museums that set out to make their national antiquities a blatant parade of their national pride.

We share with the other great museums of London a wholesomely universal outlook. It is not by a priggishly virtuous discipline that we resist nationalist self-glory; it is because we are—unless we lose all sense of proportion—completely prevented by our own great collection from indulging in such stupidity. After all, even the most distinguished British antiquities would look remarkably silly if we promoted them to take the honoured place of the pediment sculptures of the Parthenon. It is a very important thing that the great museums of London share with many other museums the duty of reflecting in a microcosm God's total creation. Both branches of the British Museum, the Science Museum, the Geological Museum, the Victoria and Albert Museum, and the National Gallery, have, put together, a cumulative purpose that ascends to the Heavens and descends to the depths of the sea, that comprehends in intention the stars on high and the innermost core of the earth, and the full story of man in this world, of his multifarious handiwork, noble and ignoble, everywhere. Our united song is Psalm 104, and not Psalm 105.

In the British Museum we are not concerned with comment upon the felicity of the chosen. We say 'Man goeth forth to his work and to his labour: until the evening' and we mean Palaeolithic man, Mesolithic man, Neolithic man, the ancient Assyrians and Egyptians, the Greeks and the Romans, the Teuton and the Celt, man in Europe, man in Asia, man in Africa, man in America, man everywhere. Sir Hans Sloane said of his collection that it was 'tending in many ways to the manifestation of the glory of God'. Its purpose is summarized by the psalmist: 'O Lord how manifold are thy works: in wisdom hast Thou made them all, the earth is full of thy riches'. This principle is still our principle. To anyone who thinks we do not sufficiently exalt the antiquities of the British Isles, I can answer only by repeating the words that I treasure and serve as our governing text:

'Behold the nations are as a drop of a bucket, and are counted as the small dust of the balance; behold he taketh up the isles as a very little thing'.

Neolithic Britain: a review

by H. N. SAVORY

BRITISH archaeological literature has often been unfavourably contrasted with that of Central and North European countries in that it lacks modern, detailed and fully illustrated books dealing with particular periods or cultural groups. Thanks to Professor Piggott, students of European prehistory now find in their hands a book* on the Neolithic cultures of the British Isles which will not suffer from comparison with the best continental manuals—in fact, is all the better for the delay in its production.

This book is a study of a major cultural grouping rather than a period: in the words of the sub-title, it deals with 'the stone-using communities of Britain in the second millennium B.C.'. This does not, in fact, completely define the subject-matter of the book, since the author has wisely excluded from his survey the Beaker and other separate grave cultures, even though he holds that 'the scanty metal equipment of the beaker-using population . . . cannot be regarded as really constituting a Bronze Age', and admits that the earliest Beaker groups entered Britain in his Middle Neolithic phase. As he suggests elsewhere, the clearest cultural dividing line that we have in 'Neolithic' Britain is that between the groups practising communal and separate burial respectively, and the latter demand a book of their own, as substantial as the present one. For the writer of such a book as this Professor Piggott has created a problem of nomenclature. The selection of a cultural grouping rather than a chronological period has undoubtedly given the present work unity and clarity within its chosen bounds, and, for its own purposes, the definition of an early, middle and late phase in this grouping is justifiable. But in the text and the accompanying chronological table the early stages of the British separate grave cultures, which conventionally define our Early Bronze Age, though not as we have seen, among the 'Neolithic' cultures, are assigned to a Neolithic *period* stretching from c. 2000 to c. 1500 B.C.: in other words, the new wine of Piggott's cultural groupings has burst the old bottles of the Three Age system.

For form, Professor Piggott has taken as his model Professor Clark's works on the Mesolithic cultures of Britain and northern Europe, to which he regards his own book as in a sense a sequel. He has striven, therefore, to give a balanced account 'of all aspects of a complicated phase of prehistory rather than a treatment of a selected element such as pottery or tomb types' and to see the first agricultural cultures of Britain as far as possible as living communities. One can only say that he has succeeded as well as our present still very uneven knowledge permits. After a survey of the geological, palaeobotanical and faunistic background to the Neolithic colonists the author defines their component cultural groups and subjects their settlements, tombs, material equipment and continental origins to a uniform analysis, in the lucid terms of which he is master; all the drawings of objects and plans of monuments, moreover, have been drawn by the author in his habitual clear and simple style, and to uniform scales: a feature which greatly contributes to the pleasure of using the book.

* *Neolithic Cultures of the British Isles*. By Stuart Piggott, Cambridge University Press, 1954. 420 pages, 12 plates, 63 text figures, 1 folding diagram. £3 10s. 0d.

The analysis of Neolithic cultural groups set forth in the book naturally consolidates views which have been developed by the author during the past 25 years, and more recently by his colleague at Edinburgh, Mr R. J. C. Atkinson. The most obvious feature is the distinction between those Neolithic communities which are thought to have introduced the western Neolithic arts from their old homes in the Mediterranean lands and those 'Secondary Neolithic' communities which are thought to represent the acculturation, under Neolithic influence, of Mesolithic communities in Britain or northern Europe. The latter are now defined primarily by a common range of stone and bone implements rather than by associated ceramic groups, among which the familiar Neolithic 'B' is now seen to be only one. The sub-groups, many of them admittedly very far from representing, as far as is yet known, complete and distinct communities, are in some cases familiar, in other cases, as in those of 'East Anglian' and 'Rinyo-Clacton' (= 'grooved') ware, comparatively novel. The available evidence for their mutual relations and chronological sequence is marshalled in a brilliant manner. Perhaps the chief impression left by this analysis is one of steadily increasing complexity and contamination of the cultural groups, brought about by their fusion with one another in areas remote from the original sources of inspiration on the Continent: by the late Neolithic phase, one is bound to wonder how much value, if at all, ceramic groups have any longer as indicators of distinct communities.

In the 'Early Neolithic', to which are assigned the early sub-groups of the Windmill Hill culture and, beginning somewhat later, the earlier Cotswold-Severn and Clyde-Carlingford chambered cairns, continental parallels are most significant, and it may be noted here that Piggott favours independent origins for the chambered and unchambered long cairns. In discussing these, he perhaps does not do sufficient justice to the evidence for long cairns in various parts of western and southern France outside of Brittany, which is now extensive and enables one to see the derivative 'horned cairns' of Sardinia in better perspective. There is a most interesting discussion of the evidence for interpreting unchambered long barrows as temporary mortuary houses for successive burials that were finally covered up and sealed by the erection over them of long mounds—a suggestion that helps greatly with the understanding not only of chambered tomb ritual but of the traces of mortuary houses found in the barrows of the separate grave people. Equally instructive, from the point of view of cultural fusion in the Middle and Late Neolithic phases, is the treatment of the long and round barrows of Yorkshire that cover 'crematorium trenches'. For here, on a horizon that extends from the Beaker into the Food Vessel phase of Yorkshire, we have the connecting link between the Wessex long barrows with platform cremations and the Clyde-Carlingford cairns with crematorium trenches, reinforced by the affinities between the Grimston and Lyles Hill sub-groups of Windmill Hill ware, and between the Sandhill sub-group of Secondary Neolithic pottery in northern Ireland and those Yorkshire sherds which recall by their decoration certain of the Neolithic funnel-beakers of Scandinavia.

Piggott's treatment of those Middle and Late Neolithic groups which are known chiefly by their megalithic tombs gains greatly, in comparison with recent abstract studies of megalithic tomb morphology, from being part of a balanced approach to British Neolithic culture as a whole. He realizes that the Cotswold-Severn and Clyde-Carlingford groups are not only primary, but being linked the most closely, by their equipment, with the more completely known Windmill Hill culture of the English lowlands, form the proper starting point for the study of the less well explored groups of passage graves and gallery graves in the Highland Zone. Against this background the Boyne passage graves with their apparent closeness to Iberian architectural traditions,

fall into their proper place as the products, during most of their development, of a late and hybrid culture in which elements derived from British Secondary Neolithic groups and the later stages of the Clyde-Carlingford and Hebridean megalithic groups combine with others derived from the Continent. These continental elements, be it noted, have rather less to do, apart from architecture, with the Iberian passage graves than even Piggott suggests. He rightly compares the poppy-headed pins typical of the tombs with those from Vila Nova de São Pedro: but the latter do not belong to the Chalcolithic Palmela culture as stated, but to the Early Bronze Age *castro* culture of Extremadura, with its degenerate Atlantic beakers and half-spool buttons of South French origin. Moreover, the segmented stone pendant from Carrowkeel (FIG. 32.26) is surely derived from the Early Bronze Age culture of Languedoc, where it is common, rather than eastern Spain, where it is practically confined to the caves at Monte Barsella, Valencia, where the grave goods show exceptionally strong southern French influence. Similarly, in discussing the origins of Rinyo-Clacton ware, which might represent another Atlantic coastal movement not unconnected with the diffusion of passage grave forms, Piggott appears to overrate the importance of the remoter parallels in Portuguese 'channelled' ware at the expense of the more convincing comparisons with western and southern French Early Bronze Age channelled decoration and S.O.M. forms: surely, too, the Rinyo-Clacton pins with loops on the shaft have their parallels in pins from the late Chassey lake-dwellings of Switzerland rather than any from Iberia.

It is fitting that the standard text-book on the Neolithic of Britain should have been written by the archaeologist who, at a tender age, nearly a quarter of a century ago, wrote the first comprehensive account of British Neolithic pottery. The author still has before him a similar span of time in which to produce the revised editions, which will undoubtedly be required as new evidence comes to light. It is probably not too much to hope that some of this edition's cultural sub-groups, defined mainly by pottery or, as with the 'Dorchester culture', merely by a type of ceremonial site, after serving their purpose as a framework for present knowledge, will be replaced by genuine cultural units on the same footing as the Windmill Hill culture, with its houses, tombs, flint mines and evidence, in the causewayed camps, for the seasonal activities of stock-raisers. Already, in the recently discovered long-house at Mount Pleasant, Porthcawl, with its drystone walling built after the manner of local long cairns, one can see how quickly attempts to make a cultural province out of Hembury, Haldon, Clegyr-boia and Lough Gur can be complicated, for the associated pottery at Mount Pleasant has little in common either with Hembury or with Clegyr-boia. To this reviewer, at least, it seems likely that fresh discoveries will tend to weaken the antithesis between primary and secondary Neolithic cultures, with the appreciation that the Mesolithic element tends to increase in both the cultural groups towards the end of the Neolithic, and that association of axe-factories with the Secondary Neolithic culture has been overstressed: for not only in Cornwall but in Pembrokeshire there must have been several axe-factories, in an area where the only known settlement is purely Windmill Hill in character and has produced an axe-head made of a Presely stone; the Presely factories, moreover, continued their activity in the Early Bronze Age, producing perforated implements of Beaker type. Finally, there is the highly debatable question of the entirely different chronological value now given to the terms 'Neolithic' and 'Early Bronze Age' by Professor Piggott. This fills the reviewer with misgivings; not only because of the confusion in language and thought that will ensue, but for the implied inclusion of the British Isles in the North European system of prehistory, and the associated tendency to exaggerate the formative importance of North European connections in the prehistory of Britain as a whole. It

is clear that a long transitional phase must be envisaged, not only between metal and non-metal using communities, but between communal- and separate-burying groups. Indeed, it is obvious from the evidence marshalled by Piggott himself that the late Neolithic communities in parts of the Highland Zone outlasted by centuries the Late Neolithic chronological phase of Piggott. Surely it would be more convenient to recognize the definitive value of the newly arrived, metal-using Beaker communities, retain the conventional content of the British Early Bronze Age and thereby recognize the greater Antiquity of British, as opposed to North European metallurgy. After all, if, instead of this, cultures surviving from an earlier age are to be definitive, why not call the *Neolithic* communities of the British Isles *Mesolithic*? If the Three Age system is to be retained at all, chronological brackets within it denominated 'Neolithic' or 'Early Bronze Age', should be made to cover large regions of Europe. The region to which the British Isles are most closely linked culturally in prehistoric times is surely western, not northern Europe, and in this region, considered as a whole, the retention of the conventional content of the Chalcolithic and Early Bronze Age is desirable. But these are minor criticisms. It is worthwhile to have waited so long for so good a book.

Index to Volumes I-XXV

From time to time enquiries are received about the possibility of publishing an Index to the first twenty-five volumes of *ANTIQUITY*, for which there is great need. No one feels the need more than we do, and we are glad to announce that such an Index is nearly ready to go to the printer. The compilation is finished and typed, and we shall shortly circulate a leaflet about it. We would remind readers that the more buyers we have the cheaper it will be. The Index has been compiled by an expert, and is itself a work of no small bibliographical value, covering the most creative decades of British archaeology.

Aberford Dykes: the first defence of the Brigantes?¹

by LESLIE ALCOCK

THE Roman road from *Legiolium* (Castleford) to *Calcaria* (Tadcaster), and its modern descendant, the Great North Road, cross the Cock Beck at Aberford², eight miles north-east of Leeds, Yorkshire (FIG. 1). The crossing is defended by a triple system of southward facing banks and ditches which total some $4\frac{1}{2}$ miles in length. The main bank, distinguished by the steepness of its scarp, still rises to 25 feet above the bottom of its ditch³, and shows in places the stonework which originally revetted it.

These entrenchments, among the most formidable of their kind in northern England, have received scant attention since Leland wrote of '2 or 3 long Diches as campos of Men of Warre'⁴. In describing them, Bogg, the romantic historian of Elmet, eked out an inadequate map with much colourful speculation⁵; Colman devoted but a single page to them⁶; the *Victoria County History* produced one inaccurate section⁷. Neglected by local antiquaries, the dykes have fared little better at the hands of archaeologists of standing, for though they were included on the *Map of Dark Age Britain*, it was not until 1952 that Dr Crawford fully publicised their extent and character⁸.

The history of the dykes has been as neglected as their topography. Bogg is followed by Mr Cowling⁹ and Mr Wood¹⁰ in attributing them to the Brigantes, but this view is based on general probabilities rather than on definite evidence. Mr Myres saw them as the defences of the British kingdom of Elmet¹¹; and Dr Crawford, connecting them with other, somewhat dissimilar, earthworks in the Pennines, suggested that they formed the setting for Arthur's battles¹². Colman on the contrary believed that they confronted the earthwork at Barwick, which he took to be a stronghold of Elmet; the Aberford dykes, then would mark the boundary of Deira, before Edwin's conquest of the British kingdom. Strangely enough no one has thought of them as the later Deiran defence-line against Mercian expeditions along the Roman road, yet this is a rôle for which they would be well designed.

¹ I am most grateful to Reverend E. A. Page and Mr A. Wellings for local advice; to the British Electricity Authority for permission to view Becca Banks; to Dr D. E. Owen for help with the geology; and to Dr V. E. Nash-Williams for reading the typescript and for helpful suggestions.

² Eadburg's ford. Ekwall, *Concise Oxford Dictionary of English Place-Names*, s.v.

³ This, and all such subsequent measurements, express the vertical heights.

⁴ Hearne's Leland's *Itinerary*, 2nd Ed., I, 43.

⁵ Edmund Bogg, *The Old Kingdom of Elmet, and the Ainsty of York*, pp. 16, 157, 164.

⁶ F. S. Colman, *A History of Barwick-in-Elmet* (Thoresby Society Publications, 1908), p. 21.

⁷ *V.C.H. Yorks.*, II, p. 57. The second ditch is almost certainly a sunken track.

⁸ O. G. S. Crawford, *Archaeology in the Field*, p. 247: the first mention of the all-important revetment! Dr Crawford informs me that his fieldwork was done in March, 1935, and was recorded at that time on the relevant O.S. 6 inch sheets.

⁹ E. T. Cowling, *Rombald's Way*, p. 148.

¹⁰ E. S. Wood, *Archaeological News Letter*, II, 3, p. 37.

¹¹ Collingwood and Myres, *Roman Britain and the English Settlements*, p. 419.

¹² *ANTIQUITY*, IX, p. 277.

Fieldwork carried out in 1953 by my wife and myself makes it possible to present an accurate map of the dykes, together with typical profiles and a detailed description. The evidence for the dating is also discussed, and it is suggested that the lay-out of the dykes points to the defence of a pre-Roman trackway, while their construction is akin to that of the Brigantian encampment at Stanwick. The Aberford dykes were, indeed, the first, as Stanwick was the last, defence-work of the Brigantes.

The Aberford dykes consist of three distinct but broadly contemporary entrenchments. The Main Dyke lies to the north of the Cock Beck; it is sometimes given the name 'Becca Banks' from the stretch west of Aberford. South of the beck are two outliers; the South Dyke, which runs parallel to the eastern section of the Main Dyke; and the Rein¹³ which runs obliquely across its eastern front from the beck to a commanding hill. The Rein and the South Dyke are both aligned on an original entrance in the Main Dyke.

I. THE MAIN DYKE

The Main Dyke is in the form of a ditch, frequently rock-cut, and a bank revetted with stone still rising 25 feet above the bottom of the ditch, which run for nearly three miles along the north side of the Cock Beck. It is sited to take advantage of a natural steepening of the ground, so that the slope in front of the ditch usually drops sharply to the beck. To the west, where the Cock Beck joins the Potterton Beck from the south, the dyke continues westwards on its original line, following the latter stream.

On the east the Main Dyke begins in the middle of Hayton Wood, as a marked bank and a broad ditch, facing south. Further west towards Aberford, the bank has been badly mutilated by farming, and the ditch has vanished. There is very little stone in the bank here because the natural rock lies deep. For lack of stone, this was probably the weakest stretch of the Main dyke, and the southern outliers were built to reinforce its front.

Immediately east of Aberford, where a shallow dry valley runs across the line of the Dyke, there is a gap about 80 yds. wide, which is clearly original. The alignment of the South Dyke and the Rein on this gap, and the existence of a natural ford opposite it (*v. infra* p. 154) argue that in fact it was the original entrance through the Main Dyke.

In the village of Aberford the ditch and bank have been completely obliterated, but they reappear to the west as Becca Banks. Here their line is sinuous and holds closely to a pronounced natural scarp overlooking the beck, so that this is almost the finest and certainly the most dominant stretch of the Main Dyke. Behind the bank lie flat fields and parkland; in front, its face drops steeply to the ditch; there is the slight lift of the counterscarp, then the ground plunges down to the swampy bottom of the Cock Beck.

Magnesian limestone lies close under the surface here, so that the ditch was largely rock-cut, and there was abundant stone for the body of the bank. Here too the bank was revetted to an angle of some 80° by courses of roughly squared stones (PLATE 1). Three or four courses of this stonework are still to be seen¹⁴ in Becca Park¹⁵, though elsewhere the Dyke has been sadly mutilated by quarrying.

Becca Banks terminate in a quarry in Becca Low Wood, but originally they were continuous with the next section, known as the Ridge. This is the best preserved length

¹³ Woodhouse Moor Rein on the O.S. 6 inch. *Rein*: a strip of land, a ridge: a division between lands or fields. *O.E.D.*

¹⁴ Crawford (*loc. cit.* note 8 above) found five courses, but between two visits on 11.7.53 and 2.8.53 the topmost course had been thrown down.

¹⁵ National Grid SE 424382.

ABERFORD DYKES

of the Main Dyke : it has suffered only slightly from the laying of cart-tracks across it. The ditch is visibly rock-cut, and the bank very steep, narrow and stony. At a typical point the ditch is 8 feet deep ; the crest of the bank rises 21 feet above the ditch, and 9 feet above the country to the north.

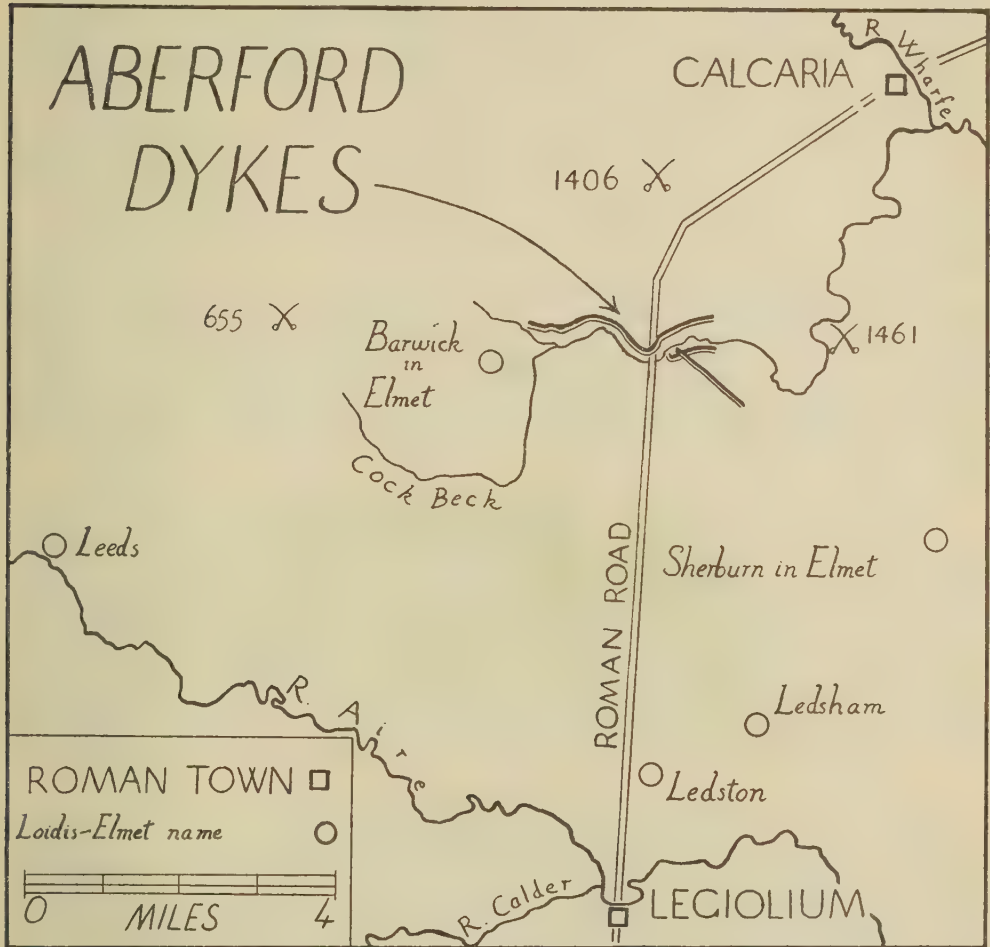


FIG. 1. THE SETTING OF ABERFORD DYKES, YORKS.
(based upon the Ordnance Map and reproduced by permission)

The Ridge forms a straight line, undulating over minor spurs and re-entrants, but clinging on the whole to a natural steepening of the ground. At the western end a tributary stream falling to the Cock Beck has carved out a marked valley. Here the Ridge turns north-west, and uses the tributary as its ditch. The dyke has been mutilated in this area by quarrying and modern ditching ; but it seems probable that the original plan was to rest its western end on this stream, which affords a reasonable defensive line.

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Whatever the original intention, the Ridge reappears across the stream, but with a markedly different character. The natural scarp, which it had followed hitherto, forms a pronounced spur. The Ridge, in order to maintain a direct line, quits the scarp, and runs up the reverse slope of the spur, with the Cock and Potterton valleys in dead ground. The crest of the spur is crossed, and the scarp rejoined 500 yards further on. Towards the west the dyke appears to be unfinished, for the bank is irregular, causeways seem to have been left in the quarrying of the ditch, and less stonework is visible. The Main Dyke finally peters out in a low bank and shallow ditch, quite unlike the wide ditch and upstanding bank of its eastern end.

Nevertheless the character of the Ridge is well maintained on top of the spur. On the summit, the ditch is rock-cut, and 7 feet deep: the bank rises 14 feet above the bottom of the ditch, and 7 feet above the country to the north. The bank is very stony; and from about 5 feet below the crest it is revetted with some six courses of slabby rock. West of this the bank rises to about 20 feet above the ditch, but the latter is only some 4 feet deep.

The Main Dyke certainly ends some 500 yards east of Potterton Bridge (National Grid SE 407382), where the valley is broader and the stream meanders along a bottom which was formerly very swampy. Bogg, however, sought to continue it up to the earthwork of Wendel Hill, Barwick-in-Elmet¹⁶, on the assumption that this was a Brigantian hill-fort. The assumption is far from proven¹⁷; and in any case Bogg's supposed connecting banks are no more than a number of hedge lynchets, strung together by the eye of fancy.

II. THE SOUTH DYKE (PLATE II)

The South Dyke runs for over a thousand yards along the southern scarp of the Cock Valley, from Oil Mill Quarry on the east¹⁸ to a point south-east of the gap in the Main Dyke, where it turns down to the river. It consists of a rock-cut ditch, some 5 feet deep, to the south, and a stony bank, rising only 5 feet above the ditch, to the north. A stretch in the middle has been thrown down, but the line of the ditch is still revealed by crop marks. The dyke is so badly sited just below the crest of the slope that the bank is overlooked by higher ground south of the ditch; yet a bank of comparable volume, sited a few yards farther south, could have dominated its approaches. It was doubtless the tactical weakness of the South Dyke which led to its abandonment in favour of the Rein.

III. THE REIN

The Rein is a fine straight line of dyke running for just over a mile from a dominating hill down to the Cock Beck opposite the entrance through the Main Dyke. It is possible to see along it from its eastern end to the point at which it plunges over the scarp to the beck. It consists typically of a ditch 7 feet deep to the south west of a stony bank from which it is separated by a narrow berm: the bank may rise 12 feet above the ditch, and 5 feet or more above the country to the north east. The bank may be lower, or infrequently higher, than that shown in the profile; bank, berm and ditch combined may

¹⁶ Bogg, *op. cit.*, p. 164.

¹⁷ Wendel Hill to-day appears simply as the bailey of a Norman motte, Hall Tower Hill; but it is not impossible that the Normans made use of a pre-existing hill-fort as they did at Almondbury, Yorks. On the other hand, Wendel Hill is not in a hill-fort area. Recent surface collecting has produced a pottery sequence back to the 12th century; but nothing earlier.

¹⁸ We agree with Crawford (*loc. cit.*) that the supposed entrenchments near Lead have nothing to do with the South Dyke, and had no military or defensive purpose.

ABERFORD DYKES

be up to 85 feet in width ; and in some places the berm disappears. But throughout these variations and irrespective of the relative ground levels inside and outside the dyke, the bank remains a strong feature.

The intersection of the South Dyke and the Rein has been mutilated by modern quarrying. It is nevertheless clear that the ditch of the Rein runs through without interruption and it follows that the Rein is the later dyke. Its layout is indeed more commanding than that of the South Dyke. But it is hard to see why the Rein should be continued down to the river, behind the existing end of the South Dyke. Nor is the tactical scheme of these outliers easy to understand. They seem intended to reinforce the weaker, stoneless end of the Main Dyke ; but a defence in depth is scarcely possible where the defenders have to retire across the natural obstacle of the Beck.

THE SETTING OF THE DYKES

From the Aire to the Wharfe, a narrow corridor of magnesian limestone runs between rocks supporting a heavier vegetation¹⁹. To the west, the edge of the coal measures and the millstone grit series forms a firm line ; to the east the Middle and Upper Permian marls outcrop more patchily, but still dominate the scene. The Middle Permian marl, for instance, is exposed as a stiff soil²⁰ likely to favour heavy forest. The magnesian limestone, by contrast, produces a friable, well-drained soil²¹ and, in consequence, a lighter vegetation. Up this limestone corridor ran the Roman road from *Legiolium* to *Calcaria* ; and spanning it, almost from side to side, lay the Aberford dykes.

In detail, the controlling influence of geology is even more striking. On the east, the Main Dyke runs off the limestone and on to the marl for some distance before stopping. Further east, its flank is protected either by outcrops of marl or by patches of boulder-clay, equally favourable to forest growth. The Rein in the same way runs south-east until it reaches the outcrop—here, admittedly, discontinuous—of the marl : it too rested, then, on dense forest. Local geology rules again at the western end of the Main Dyke, where its defensive character deteriorates. This change occurs just at the point where the mud-stones of the coal measures, which in general lie more to the west, are first exposed. These mudstones encourage dense woodland, and there can be no doubt that a combination of swamp and heavy forest protected the west flank of the dyke. The South Dyke alone seems to have ended, on the east, ‘in the air’ ; a further reason for its early replacement by the Rein.

The strategic importance of north-south movement in this area between the Pennines and the Vale of York is shown by the battles known to have been fought in the neighbourhood (FIG. 1). Of these, that at Spen Hill in 1406 was no more than a skirmish in a revolt against the House of Lancaster. But at Winwaed²² in A.D. 655 Oswiu of Northumbria crushed decisively the alliance between Penda of Mercia and the Welsh princes, and asserted Northumbrian supremacy over the North Midlands. The battle of Towton Field in 1461 is even more instructive. The troops of Lancaster had advanced across the river Wharfe ; but in the face of a superior Yorkist force, they were unable to retire across the Cock Beck, save over the causeways formed by the corpses of their own slain.

¹⁹ *Geological Survey, One Inch*, Sheets 70, 78 (1951 edn.) ; details checked on mss. 6 inch sheets at the Geological Survey. Mr Edwards, of the Survey, very kindly discussed the geology with me.

²⁰ Edwards, Mitchell, and Whitehead, *Geology of the District North and East of Leeds*, p. 41.

²¹ *ibid.*, p. 34.

²² Identified with Winn Moor, 4m. NE. of Leeds.

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THE DATE OF THE DYKES

There are no archaeological finds from the Aberford Dykes. Evidence for dating them must therefore be sought in the details of their construction, in their layout and in the history of north-east England. One suggestion may be rejected outright—that the dykes defended the British enclave of Elmet²³. As the map (FIG. 1) shows, a significant group of Loidis-Elmet place-names lies outside the Aberford defences²⁴. The contrary view, that the dykes are a Northumbrian defence against the British²⁵ is equally unlikely; for even if Wendel Hill proved to be a Celtic hill-fort occupied as late as the 6th and 7th centuries A.D., the Aberford dykes were clearly never planned to contain it.

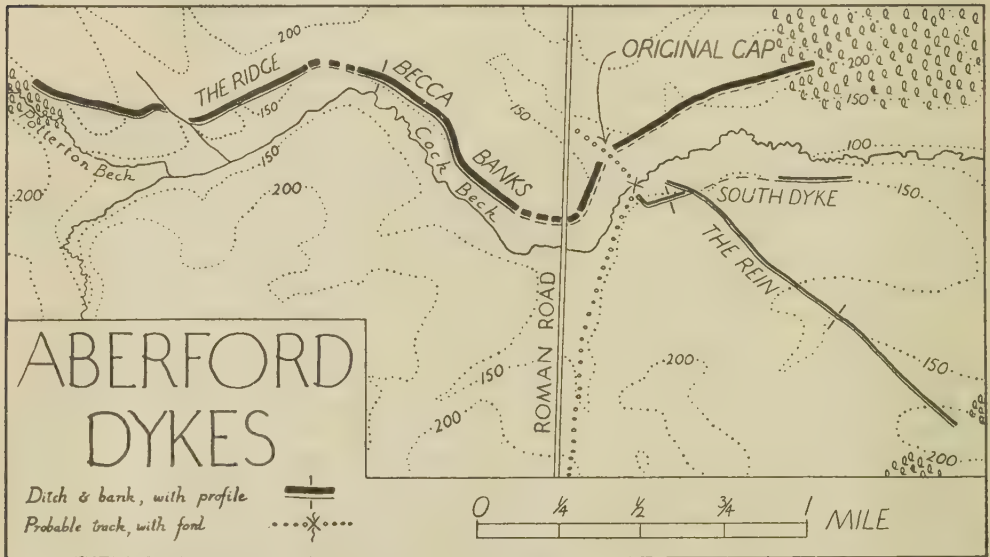


FIG. 2. THE LAYOUT OF ABERFORD DYKES
(based upon the Ordnance Map and reproduced by permission)

The Dykes seem, indeed, designed to secure the line of the Cock Beck, where it was forded by the Roman road. If they are laid out with reference to the road, then they must be later than its building; and since they cannot have been dug under the *Pax Romana* they must be post-Roman. The battles at Winwaed and at Hatfield, further south, show that the West Riding was in dispute between Northumbria and Mercia in the 7th century. The Roman road, prominent even to-day as a high ridge, would make an obvious route for Mercian raiders; and the Aberford Dykes could have formed the Southern defence of Deira against such incursions.

This attractive hypothesis fails as soon as we consider the construction of the Main Dyke. At two points—on Becca Banks (PLATE I, A) and where the Ridge crosses the spur above Potterton Beck—there are still signs of an original stone revetment to the bank: and the stony nature and steep scarp of Becca Banks and the Ridge argue that this revetment

²³ Myres, loc. cit.

²⁴ Ekwall, op. cit., s.vv.; Mr Doyle-Davison has kindly commented on these attributions.

²⁵ Colman, loc. cit.

ABERFORD DYKES

was originally continuous. Now of the many Anglian earthworks which have been investigated²⁶ none had a revetment, whether of turf or stone. On the contrary, stone revetments are characteristic of Iron Age hill-forts and Sir Mortimer Wheeler has recently uncovered one in the bank surrounding the Brigantian camp at Stanwick, in the North Riding of Yorkshire²⁷.

These Iron Age sites are, of course, camps and not fortified lines: but linear defences, though rare, are not unknown in pre-Roman Britain. The gigantic ditch, with a slight bank on either side, at Beech Bottom, near Verulamium²⁸, shows that the principle of

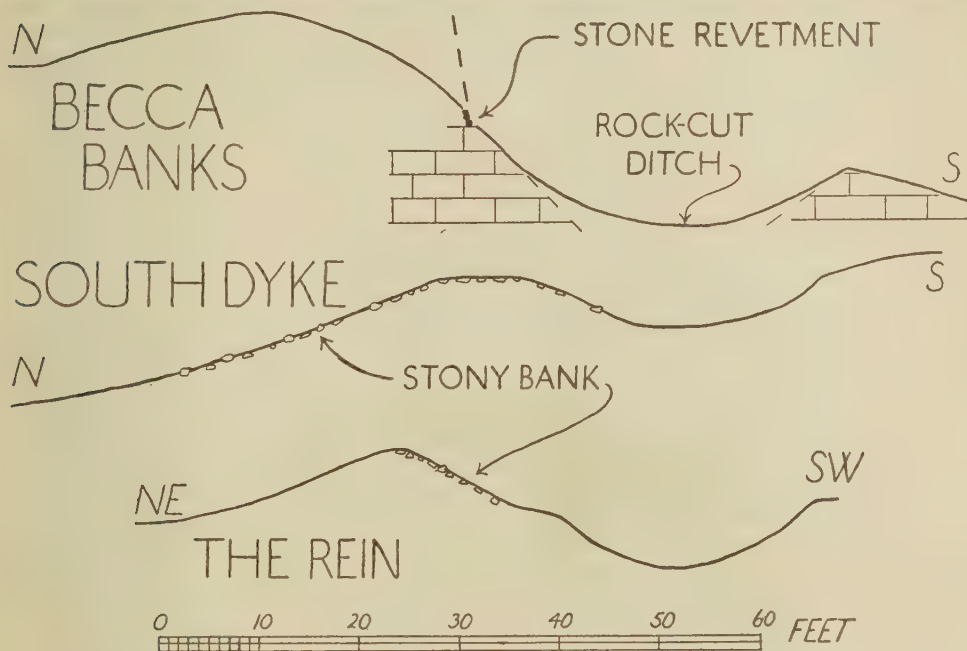


FIG. 3. ABERFORD DYKES: TYPICAL PROFILES

such defences was known by the 1st century B.C. More relevant to the present case are the defences of Belgic Colchester. The bank of the Lexden Dyke, for instance, constructed early in the 1st century A.D. to protect Cunobelin's capital, was shown by excavation to have had a turf revetment²⁹. This clearly is the same building technique as that of the Aberford Dykes, under different geological conditions. We have already seen the technique used at Stanwick by the Brigantes: and we need no longer hesitate to ascribe the Aberford Dykes to that large and warlike tribe.

This argument from the construction of the dykes is reinforced by a study of their lay-out (FIG. 2). The intersection of the Main Dyke and the Roman road has been obliterated by the modern village of Aberford, so that nothing can be said of their relationship.

²⁶ There are accessible accounts in *ANTIQUITY*, III, pp. 135-54.

²⁷ *Ant. J.*, XXXII, p. 3 and Pl. v.

²⁸ *ANTIQUITY*, VII, pp. 21-35.

²⁹ Hawkes, C. F. C., and Hull, M. R., *Camulodunum*, pp. 12-13. Mr Hull very kindly supplied me with unpublished drawings of this and other Belgic turf-revetted dykes.

But the position of the original entrance and the lay-out of the southern outliers suggest a track to the east of the road. This track would have descended a shallow valley to the Cock and, passing through the entrance, would have ascended a similar valley to the north. These valleys are floored with the solifluxion soil known as Head³⁰, and were probably only lightly forested. The Cock itself could be forded, opposite the entrance, by a bank of hard gravel, comparable to that at the Roman ford (FIG. 2). Such a track would not have been used once the Roman road had been laid: it must precede the Roman conquest of Yorkshire, and it substantiates the case for the Brigantian origin of the Dykes.

The Aberford Dykes, then, are most convincingly explained as a Brigantian work, thrown up across a prehistoric trackway to block the advance of an enemy from the South. The most likely enemy is, of course, the Romans. The Dykes represent a northward contraction of the area of Brigantian control as demonstrated by the distribution of their coins³¹. This contraction may have been determined by the need for a good defensive line; for the Cock has a narrower and more easily controlled valley than either the Aire or the Don; but it is more likely that it was enforced by Roman supremacy in Lincolnshire and Southern Yorkshire. The Ninth Legion was established at Lincoln by about A.D. 47.

Archaeology, then, determines the broad setting of the Aberford Dykes; for their closer dating we must turn to history. The story of the resistance and defeat of the Brigantes is scattered piecemeal through the pages of Tacitus. Their first contact with the Romans appears to have been a mere marauding expedition on the flank of the advance into Wales, in A.D. 47-8: for, 'a few having been killed, the trouble was quickly quelled'³². Remembering that the closest parallel for the Aberford Dykes are those which defended Cunobelin's capital, it is tempting to see them as the work of Cunobelin's son, Caratacus, when in A.D. 51 he fled north to continue the struggle against Rome. But it is impossible to read this into Tacitus' compressed account—'*cum fidem Cartimanduae reginae Brigantum petivisset, victus ac victoribus traditus est*'³³. Nor was there any sign at that date of a national resistance party among the Brigantes, for even in A.D. 57-8 Cartimandua's consort Venutius, on whom Caratacus' mantle of military distinction had fallen, is described as one 'long faithful' to Rome³⁴. At that time, indeed, Venutius' attitude changed, partly as a result of a quarrel with his wife: and by A.D. 69, influenced by 'hatred of the Roman name', as well as by personal issues, he called in help and revolted. The pro-Roman Cartimandua was rescued, with difficulty, by a force of auxiliaries: but 'the kingdom was left to Venutius'³⁵.

This revolt was probably the occasion for the building of the Aberford Dykes. Certainly a later date is impossible, for by A.D. 71-4 the Ninth Legion had by-passed or overcome these defences, and had established its base at York³⁶. Thereafter the scene shifts to Stanwick, the great tribal encampment which witnessed the final defeat of the Brigantes.

POSTSCRIPT

The case advanced here for attributing the Aberford Dykes to the Brigantian resistance to the Romans has now been strengthened by the publication of an independent but parallel instance—the Siluran defence of the Cotswolds. Mr and Mrs O'Neil have argued that the invaders advanced not along a Roman road but along a prehistoric trackway. They were, it seems, opposed by the stone-revetted dyke on Minchinhampton Common, which may be the work of Caratacus. (B. H. St. J. O'Neil and H. E. O'Neil, 'The Roman Conquest of the Cotswolds', *Arch. Journ.*, CIX, 23-38).

³⁰ *Geology* . . . *East of Leeds*, p. 60.

³² *Annals*, XII, 32.

³³ *ibid*, 36.

³¹ D. Allen, *Archaeologia*, xc, pp. 40-3; Map VIII.

³⁴ *ibid*, 40.

³⁵ *Histories*, III, 45.

³⁶ *J.R.S.*, XVIII (1928), 98-9.

Minoan Chronology Reviewed

by R. W. HUTCHINSON

THE system of Minoan chronology proposed by the late Sir Arthur Evans remained almost unchallenged for many years. The first hint that it might not be equally valid for all sites in Crete was given by the excavations of Dr Joseph Hazzidakis at Tylisos where he found three archaeological strata, the first corresponding to Evans' Early Minoan I, II and III and Middle Minoan Ia, the second to M.M. Ib, M.M. II, M.M. III and L.M. I and the third to Late Minoan III. On the basis of this M. L. Franchet proposed a new classification consisting of Early Neolithic (representing a site examined by Franchet himself), Late Neolithic, Bronze I, II, III and IV and Iron Age¹. This was tacitly ignored by most scholars, partly because the author was an authority on ceramics rather than on Aegean prehistory, partly because of the cavalier fashion in which this theory had been expressed. That Franchet's opinions were not entirely without substance, however, was demonstrated later by L. Åberg in the far more serious and detailed criticism of Evans' chronology expounded in his *Bronze-zeitliche und Früheisen-zeitliche Chronologie*, where he divided Evans' Minoan periods into Pre-Palatial (E.M. I to M.M. Ia), Early Palatial (M.M. Ib to pre-earthquake M.M. IIb) and Late Minoan or Late Palatial (including Evans' post-earthquake M.M. IIb). Within these three periods Åberg considered the various stylistic subdivisions to be contemporary. Åberg's complaint that the earlier periods were not well stratified at Knosos was replied to by Pendlebury who in his *Archaeology of Crete*, published in 1939, tabulated significant and un-mixed deposits at Knosos of E.M. I, II and III, M.M. Ia, M.M. Ib, M.M. IIa, M.M. IIb, M.M. IIIa and M.M. IIb pottery. So far Evans' chronology had been shrewdly attacked but adequately defended so far as Knosos was concerned, though Pendlebury had admitted that certain categories were luxury wares and might be absent from other parts of the island.

It had already become evident that the application of Evans' typology without reservation would be rather like the dating of all varieties of Victorian china by the successive phases of wares from the Derby factory. Further, Evans had relied for his absolute dates on the chronology elaborated for Egypt by Edouard Meyer. But the dates for Egypt and Mesopotamia had to be cut down, and all Minoan dates in consequence.

In 1947 Professor Claude Schaeffer published his *Stratigraphie comparée et Chronologie de l'Asie Occidentale*, an admirable synthesis of the evidence both from archaeology and from inscriptions bearing on the relative and absolute dating of the prehistoric cultures of the near East. On archaeological evidence he supported Smith's date of 1792 for Hammurabi of Babylon against Albright's 1728 or Poebel's 1715².

In 1948 I summed up in *ANTIQUITY* the current position of Minoan chronology, and the following year P. V. Milojević, in an appendix to his article on Serbian prehistory,

¹ *Tylissos à l'Époque Minoenne*. Introduction. Paris 1921. Both spellings are correct but personally I prefer the ancient varieties, Tylisos and Knosos, to those more current. I must confess, however, that I have spelt Hammurabi and Amenemhat, etc., in current forms.

² Schaeffer, loc. cit. pp. 30, 31; S. Smith, *Alalakh and Chronology and A.J.A.* 1945, p. 1 f. Albright, *B.A.S.O.R.*, 1948, p. 126. A. Poebel, *I.N.E.S.*, 1942, pp. 247, 450.

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stated his views on Aegean chronology, accepting for Hammurabi the date proposed independently by Smith and Ungnad, and relying for his Egyptian dates on the revision proposed by Dr H. Stock³.

Stock's revision of Egyptian Chronology is as follows :—

2900 B.C.	_____	
	1st and 2nd Dynasties	_____
2675	_____	
	3rd Dynasty	_____
2620	_____	
	4th and 5th Dynasties	_____
2360	_____	
	6th Dynasty	_____
2200	_____	
	8th Dynasty	1st Intermediate Period
2180	_____	
2136	9th Dynasty in Heracleopolis	_____
2115	_____	11th Dynasty in Thebes
	10th Dynasty in Heracleopolis	_____
2099	_____	
	11th Dynasty over all Egypt	_____
1994	_____	
		12th Dynasty
1781	_____	

For the Mesopotamian dates Matz follows Ungnad who had independently arrived at the same conclusion as Smith concerning the date of Hammurabi.

Ungnad's main dates are as follows :—

3100	_____	
2850	Jamdat Nasr culture	_____
2839	_____	Archaic period Kish
2700	_____	Recent period Kish
2550	_____	
2378	Early Dynastic period in Sumer	_____
2237	Gutium Rule	_____
		Kingdom of Akkad
2182	_____	
2127	_____	
2113	_____	3rd Dynasty of Ur
2112	_____	
2055	5th Dynasty of Uruk	_____
2016	_____	1st Dynasty of Isin
2006	_____	
1894	1st Dynasty of Babylon	_____
1880	_____	
1792	Hammurabi's accession	_____
1595	_____	

³ V. Milojević, *B.S.A.*, 1949, p. 300 ff. ; Ungnad, *W.P.Z.*, 1949, p. 109 ; Stock, *Analecta Orientalia*, 1949.

MINOAN CHRONOLOGY REVIEWED

Milojčić quotes Reisner's conclusion that the Egyptian stone vases found in sub-neolithic contexts at Knosos were of forms that did not develop before the reign of Khasekhemui, the last king of the second dynasty, and he therefore infers that the beginning of the first Early Minoan period could not have antedated 2700 B.C. and might more probably be placed between 2650 and 2600 B.C., and that the Early Helladic I period, which began during the E.M. II period on Aegina could scarcely have started before 2600 B.C. at the earliest. Reisner's evidence on the stone bowls from Egypt supports Pendlebury's brilliant suggestion that the Egyptian influences remarked in the Early Minoan culture of the Mesara were introduced by refugees not from the conquests of Menes, as Evans had supposed, but from those of Khasekhemui⁴.

Matz's Aegean system which agrees on many points with that of Milojčić is as follows :—

	<i>Egypt</i>	<i>Crete</i>	<i>Troy</i>	<i>Cyclades</i>	<i>Hellas</i>
2900	1st and				
2800	2nd Dynasties	Neolithic			
2700		and			
2675	—————	Sub neolithic			
	3rd Dynasty				
2620	—————				
2600	4th and				Thessalian A
2500	5th Dynasties	E.M. I.	I		—————
2450				Pelos	
2400		Pyrgos	—————	group	E.H. I
2360		culture.			Thessal-
2350	6th Dynasty		II		ian B
2300	—————				
2250					E.H. II
2200		E.M. II			—————
2150	1st Intermediate		III and	Syros	
2115	—————	E.M. III	following cities	group	—————
2100	11th Dynasty				
2000	—————				E.H. III
1994		M.M. I			—————
1900	12th				
1830	Dynasty	M.M. IIA			M.H. I
1800	—————				
1781		M.M. IIB			
1750	—————				
1700					

The following year Professor Friedrich Matz published his revision of Aegean Chronology in *Historia*⁵, also relying on Stock's dates for Egypt and on a late date for Hammurabi of Babylon without voting decisively on the question whether Smith, Albright, or Poebel was most correct on this point⁶.

⁴ *Archaeology of Crete*, p. 55, footnote 2.

⁵ *Historia*, 1950, Vol. I, No. 2, p. 173 ff.

⁶ C. F. Smith, *Alalakh and A. J. A.*, 1945, p. 1 f, Albright, *Bulletin of the American Schools of Oriental Research*, 1948, p. 126. Poebel, *Journal of New Eastern Studies* I, p. 247, and II, p. 565, it should be remarked that all authorities seem to agree that the Khorsabad King List is a reliable document for Assyria, the damaged Nassouhi King List less reliable, and King List A is a reliable document for Babylonia though the exact degrees of liability may be open to question.

Recently Professor Doro Levi has published a splendid series of M.M. IIA vases excavated at Phaistos in a deposit immediately overlying one of the late neolithic periods⁷ and has suggested that the latter lasted almost up to 2000 B.C. and that the early Minoan period was only very short and transitional. It would be unfair to condemn this theory, which Levi has expressed with due caution, until the evidence is published more completely, but it may be remarked that the new deposit is near the slope of the hill and so subject to denudation, and further that when a great palace is constructed the preliminary terracing is apt to remove earlier deposits. The Palace of Minos is also deficient in Early Minoan deposits and the deficiency there also may be due to either of these alternative explanations.

The Italian excavators of Phaistos have also stressed their inability to distinguish between M.M. I and II deposits. A certain amount of overlapping of these styles had been admitted by Pendlebury, but it is no help to omit with Åberg the distinctions between M.M. Ia and b or M.M. IIA and b, since these are much clearer than the distinction between M.M. Ib and M.M. IIA. The real difficulty was pointed out by Dr Sidney Smith with reference to the material from the so-called tholos tombs of the Mesara, where mature M.M. Ia, M.M. Ib and M.M. IIA ware may be found in the same tomb, and with reference to the Cretan exports to Egypt where it seems evident that M.M. IIB pottery began to be introduced some twenty years before M.M. IIA pottery had died out⁸.

Professor Banti's evidence for the overlapping of M.M. I and II and even III pottery at Phaistos is clear and unmistakable⁹ and Pendlebury had already in 1938 realised that in Central Crete E.M. III seemed to overlap with M.M. I, and that the earliest style of M.M. Ia was almost confined to Knosos, and had cited the persistence of certain patterns from E.M. II to M.M. Ia¹⁰.

In her summary of the Phaistos evidence later reviewed and criticised by Dr N. Platon, Professor Banti argued that the pottery from the floor of the first palace when it was destroyed should not represent any great interval of time, that the period of destruction was about 1600 B.C., just before the L.M. Ia period, and yet this floor deposit contained pottery belonging to Evans' M.M. Ib, M.M. IIA, M.M. IIB, M.M. IIIA and M.M. IIIB periods. Was this due to the unreliability of the Knosian sequence or did some styles last later on the Mesara? In her opinion the whole life of the Palace at Phaistos could not have exceeded a hundred and fifty years, and might not have lasted much more than fifty years.

An outside check on the Middle Minoan period is provided by the excavation at Alişar in Cappadocia. The chalcolithic stratum on that site Alişar O has been dated by the carbon 14 method as 2519 B.C. \pm 250 years¹¹. Schaeffer in 1952 dated the copper age 2300-2100, the succeeding stratum Alişar III at 2100-1950 or 1900, and Alişar II (which despite its numbering starts later than III), as 1900-1600 B.C.¹² Some inscribed clay tablets of the 'Cappadocian' type which Albright dates 1920-1876¹³ were found in

⁷ *Illustrated London News*, 19 January 1952.

⁸ L. Pernier and L. Banti, *Festos II*, passim, and S. Smith, *A. J. A.*, XLIX, 1945, p. 1 ff.

⁹ *Annuario d. Scuola archaeologica di Atena* 1942, p. 9 ff.; though this evidence must not be allowed to obscure Professor Levi's recent discovery of what appears to be pure M.M. IIA pottery just above a neolithic deposit referred to above nor the fact that except under Room XXVII the prepalatial pottery seems to be nearly all M.M. Ia.

¹⁰ *Archaeology of Crete*, pp. 94 and 104.

¹¹ C. B. M. McBurney, *ANTIQUITY*, 101, p. 35 f.

¹² *Stratigraphie comparée et Chronologie de l'Asie Occidentale*, pp. 321-27.

¹³ Albright, loc. cit.; Smith in *Alalakh* dates them 1970-1870.

Alişar II along with some two-handled beakers with crinkled rims, strongly reminiscent of some M.M. Ib vases from eastern Crete (a vase in silver from a grave near Gournia, and examples in clay from graves on Pseira and on a small island off Mallia¹⁴, in the 'Kouloura' houses at Knosos and in M.M. I graves on Monasteriako Kephali (unpublished). A similar beaker was also found in the IV A stratum at Boghaz Köi¹⁵. Schaeffer equates Alişar II, Tarsus II, Boghaz Köi IV and Middle Ugarit II dating them 1900–1750/1700. This would suggest for the beginning of M.M. Ia a date not much earlier than 1950 or much later than 1850.

Minoan exports of the M.M. I period are hard to find but tomb 6A at Lapithos in Cyprus contained a typical early M.M. Ia spouted jar of the Knosian type together with Cypriote red polished vases of the type designated by Gjerstad R.P. II and III and by Dikaïos Early Cypriote II and III¹⁶. Similar bowls were found at a temple of Tôd in Upper Egypt associated with four chests inscribed with the name of Amenemhat II (1936–1903 B.C. according to the old accepted date but perhaps preferably 1929–1895). This would agree well enough with a date about 1900 B.C. for the beginning of M.M. Ia or an earlier date but not with anything much later.

The only M.M. Ib export I remember recorded on a site in the Levant is one found by M. Dunand at Byblos in level II dated 2100–1900 B.C. by the excavator (Middle Bronze I)¹⁷.

In a later deposit of M.M. II contemporary with the Middle Empire of Egypt, Dunand discovered fragments of a bridge-spouted jar of M.M. IIA type, and fragments of 'Kamares' cups. This deposit is dated Middle Bronze II and (1900–1750 B.C.) according to Schaeffer¹⁸.

Additional evidence of Minoan exports of M.M. II vases to Syria was produced by the French excavations at Ras Shamra where funerary deposits of the Middle Ugarit II period (contemporary with Senusret II and Amenemhat III of Egypt and dated 1900–1750 B.C. by the excavator) produced part of a M.M. IIA eggshell cup and other fragments of the same style¹⁹. Another M.M. II sherd was discovered at the excavations at Mishrifi Qatna²⁰. Caveau LVII in the Middle Ugarit II strata produced Babylonian cylinders of the type current in Babylonia during the reign of Hammurabi and parallel to the famous cylinder found in tholos B at Platanos. The Ras Shamra stratigraphy suggests that Hammurabi should be later but not much later than Amenemhat III. For the evidence in favour of dating Hammurabi's accession to 1792 rather than 1728 with Albright or 1715 with Poebel I must refer readers to Smith's article in the American Journal of Archaeology²¹. Briefly, however, his arguments may be summarised as follows:—

The M.M. IIA sherds found at Haraga in Egypt were found in a dump deposited by workmen engaged on the tomb of Senusret II, a dump containing no objects datable to the reigns of Senusret III or Amenemhat III, though these were common on this site. (The one sherd of Tall al Yahudiah ware is no exception because this could be an import

¹⁴ Åberg, loc. cit.

¹⁵ Schaeffer, loc. cit., fig. 182.

¹⁶ V. R. Grace, *A. J. A.*, 1940, pl. II, Schaeffer, loc. cit., fig. 196 (15–17), and p. 336. M. J. Vandier, *Syria*, 1937, p. 72.

¹⁷ Dunand, *Fouilles de Byblos I*, p. 311, no. 4170, Schaeffer, loc. cit., p. 66.

¹⁸ Schaeffer, *Stratigraphie Comparée*, pp. 65, M. Dunand, *Bulletin de Musée de Beyrouth*, 1939, p. 77.

¹⁹ Schaeffer, *Ugaritica I*, pp. 54–6, figs. 43, 44, 47, 49, and *Stratigraphie Comparée*, p. 16 ff.

²⁰ Schaeffer, loc. cit., p. 117.

²¹ *A. J. A.*, XLIX, p. 7.

from Syria where the fabric occurs earlier.) This suggests a round date of 1850 for the production of M.M. 11a pottery. M.M. 11b sherds were found at Lahun in a settlement of workmen engaged on a pyramid of Senusret II. This tomb must have fallen into disuse when the 13th Dynasty began to lose control and the worship of the deified kings of the 12th Dynasty discontinued perhaps about 1750 B.C. and in any case before 1700 B.C.

At Abydos a transitional M.M. 11a-b vase was found in a grave that could be either 12th or 13th dynasty but was one of a group of six graves all constructed in a fairly short period. One of the other groups contained cylinders bearing the names of Senusret II and Amenemhat III, cylinders of an official type only used during the reigns of the monarchs mentioned. The official concerned therefore should not have held office before 1877 or after 1837. Again we have a date for the transition of M.M. 11a and b about 1830.

This dating agrees well enough with the evidence from Egyptian objects found in Crete. The amethyst scarab from the earliest deposit in the cave at Psychro might be 13th dynasty to judge from its legs, 12th dynasty to judge from the hard material employed. The diorite statue found in a pure M.M. 11b deposit in the Central Court of the Palace of Minos cannot by its inscription be earlier than the 11th or later than the 13th dynasty. The carelessness of the writing suggests end of 12th or early 13th century.

Tholos B at Platanos in the Mesara contained three Egyptian scarabs; the earliest (dated First Intermediate Period by Hall) is dated by Smith as perhaps not earlier than 12th Dynasty. A second (also assigned to the First Intermediate Period by Hall) is also dated 12th century by Smith. The third it is suggested might be as late as the 13th (though it might be earlier). The same tomb held a Babylonian cylinder of the type current in Hammurabi's reign. The pottery is similar to M.M. 1a.

This agrees quite well with Smith's date of 1792 for the accession of Hammurabi, and also with the statements of Professors Banti and Levi concerning the impossibility of distinguishing M.M. I from M.M. II strata at Phaistos and on the Mesara generally. It does not, however, agree with Poebel's or even Albright's date for Hammurabi which would imply that mature M.M. 1a pottery was about a hundred years later than M.M. 11b pottery. Against such an assumption, never made but implied by Albright's and Poebel's dates we may cite the stratification of the Royal Pottery Stores at Knossos where a M.M. 11a deposit overlies a M.M. 1b stratum. If Poebel's date for Hammurabi is therefore difficult to reconcile with the Cretan and Egyptian evidence what is its gain? It should be emphasized that people like Schaeffer and myself who are inclined to favour Smith's dating are not thereby rejecting the Khorsabad king list, which seems to me an admirable document, but Poebel's interpretation of the gaps in the king list, which is quite another matter, and which is dependent not only on assuming that the reigns of Enurta-Tukulti-Assur and Mutakkil Nusku were fractions of a year but also on the translation of an Assyrian phrase formerly regarded as implying an indefinite period, to mean the beginning of kingship. If the old translation was right we should be left with not two but ten kings with reigns of indefinite length after the 18th century B.C.²²

Poebel's system saves the date assigned by Essarhaddon to the restoration of the Anu-Adad temple by Shamshi Adad I, but he pays too high a price for this advantage in my opinion. Poebel's date for Assurballit is 1355-1320 B.C. yet we know that the King

²² The assumption by Rowton (*loc. cit.*) that these two kings never held the eponymous magistracy does not seem inevitable, and even if true might be capable of another explanation. Rowton also tried to support Albright's chronology by identifying the period between the fall of Ibin Sin and that of Nineveh with Ctesias' 1300 years of Assyrian rule (*J.N.E.S.*, 1951, pp. 184-204).

PLATE I



a. ABERFORD DYKES: REVETMENT IN BECCA BANKS



b. CLOSE-UP OF REVETMENT (Ruler = 14 ins.)

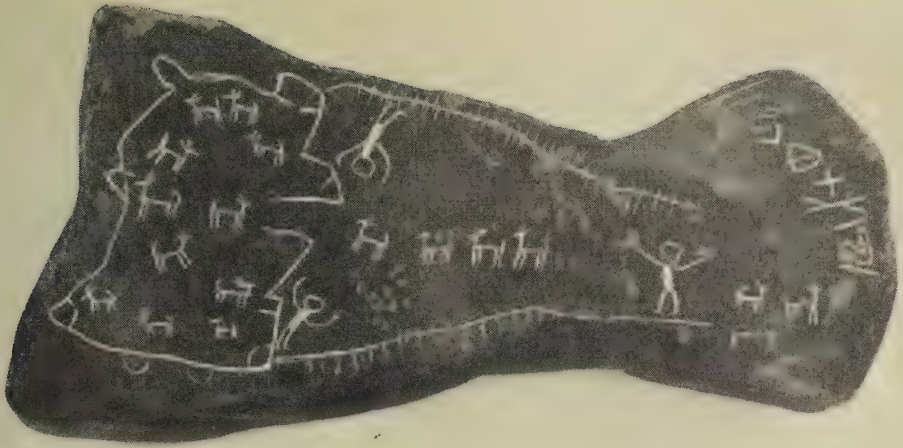
PLATE II



a. THE CORNER OF THE SOUTH DYKE AT THE WEST END LOOKING EAST ON THE OUTER (SOUTH) SIDE.
THE TREE ON LEFT IS THAT ON LEFT OF THE VIEW BELOW (*b*)



b. THE WEST END OF THE SOUTH DYKE AT ABERFORD, LOOKING WEST ON THE INNER (NORTH) SIDE



a. CONTEMPORARY PLAN OF A 'DESERT KITE'
(see p. 166)



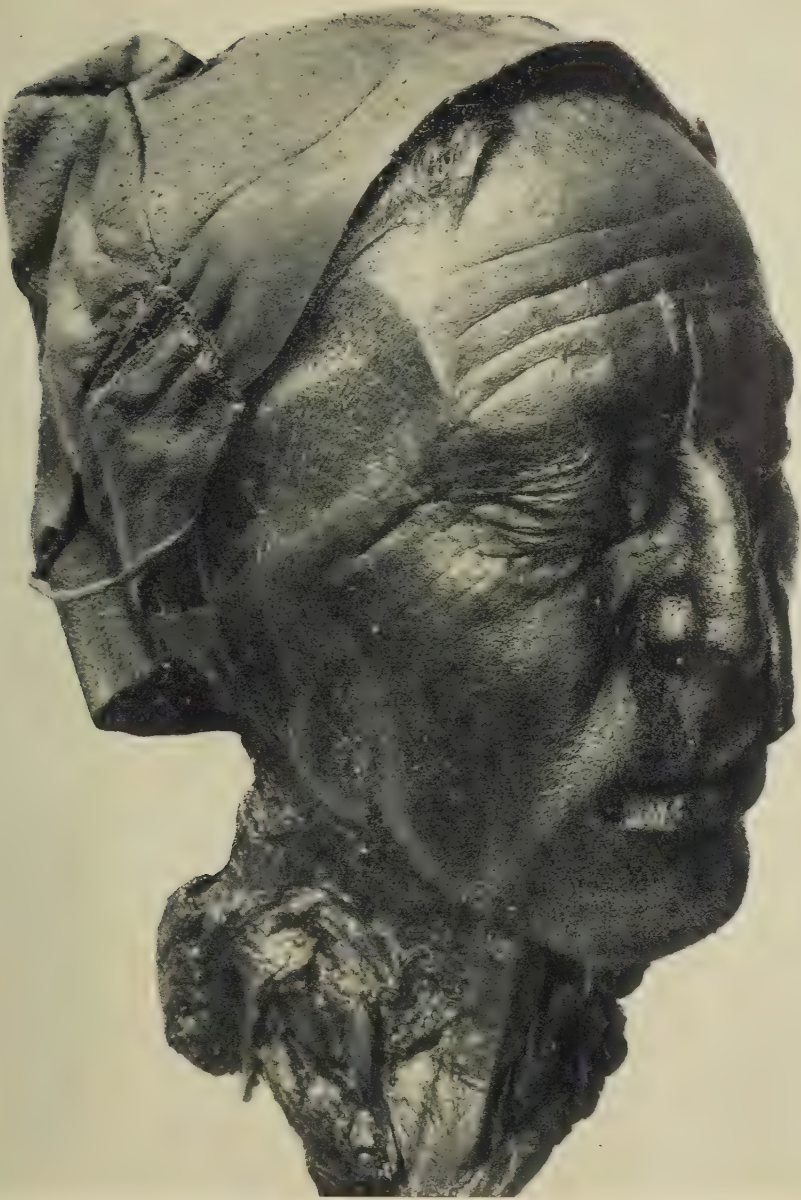
b. TYPICAL 'KITE' IN THE SYRIAN DESERT
R.A.F. photo, Crown copyright

PLATE IV



THE RECLINING DEITY, IN RELIEF

PLATE V



THE FACE OF TOLLUND MAN

PLATE VI



THE LONG MYND, LOOKING N.E. TOWARDS CHURCH STRETTON
R.A.F. photo, Crown copyright reserved

PLATE VII



NEARER VIEW OF THE LONG MYND, SHOWING ANCIENT TRACKS
AND FIELD-BANKS



was addressing Akhenaten as brother before 1358 in terms that he would hardly have used before his defeat of Mitanni and the death of Dushratta. A few years therefore at least, not Poebel's nought, should be allowed for the reigns of Enurta-Tukulti-Assur and Mutakkil Nusku.

There is a further difficulty in the synchronisms with the Kassite kings. The thirty-second Kassite king Adad-Shum-Nasir and his Assyrian foe Enlil-Kudur-Usur fell in battle together in 1200 B.C. in the five hundred and forty-fourth year of the dynasty according to Babylonian records²³, but the first year of that dynasty coincided with an invasion by the Kassites of Eastern Babylonia halted by Samsuiluna of Babylon in the eighth year of his reign, thus giving 1744 for the foundation of the Kassite dynasty, 1749-1712 for Samsuiluna and 1792-1750 for Hammurabi.

There is one more difficulty to be resolved. Tiglath Pileser I gives 701 years interval between his own restoration of the Adad temple and that of Shamshi Adad III son of Ishme Dagan. This would give the date of 1815 in Assyrian lunar years or 1794 in Julian years for that King. Smith suggests that Tiglath Pileser has made a mistake between Shamshi Adad III son of Ishme Dagan and Shamshi Adad I father of Ishme Dagan, dated before 1810 (Assyrian years) according to Shalmaneser I. Albright also rejects Poebel's dating but suggests 1728 B.C. for Hammurabi's accession basing his date on the identification of a certain Yanti Ammu prince of Byblos mentioned in the Mari records with an Enti of Byblos referred to on two Egyptian seals and on a stele from Mari showing that the latter was a contemporary of Neterhetep I of Egypt (1740-1729 B.C.). Now Zimri Lim 'the successor of Yanti Ammu' ruled Mari for thirty years until his city was captured by Hammurabi of Babylon in the thirty-second year of his reign. Albright therefore suggests 1740-1730 for the reign of Yanti Ammu, 1728 for the accession of Hammurabi of Babylon and 1697 for the latter's capture of Mari²⁴; this date was supported by Rowton who considered the Khorsabad list to be based on an eponym list, and the margin of error likely to be small²⁵.

This is slightly better than Poebel's date but still does not allow room for the Cretan-Egyptian synchronisms unless we are to change the date of the accession of Senusret II accepted by Albright who gives 1720 as the approximate date when the 15th Dynasty began to lose control of lower Egypt. That the accepted dates for the 12th Dynasty are not far out is also suggested by the radio carbon dating 1669 ± 250 B.C. for the funeral ship of Senusret III (dated 1698 by the orthodox dating) but the radio-carbon dating, of course, would agree with Albright's date also.

At Tall Atchana, the ancient Alalakh, in the Orontes valley the excavations of Sir Leonard Woolley produced evidence that is relevant to Minoan chronology, though it is less helpful than that of Egypt and Ras Shamra. (See *Antiquaries Journal* 1938, 1939.)

The earliest level here, VII, contained the palace of a certain king Yarim Lim who was ruler of Yamkhad and apparently controlled the Aleppo district as well as the Orontes valley, and whose palace must have been built after Shamshi Adad I's march to the Mediterranean but before the death of Rim Sim of Larsa; in other words between the tenth and thirtieth years of the reign of Hammurabi of Babylon (that is 1782-1762, according to Smith, 1718-1698 according to Albright, 1705-1685 according to Poebel).

²³ Rowton, loc. cit., given 1183 for the date of this battle based on the interval of 155 years since the death of Assuruballit according to the Khorsabad King List.

²⁴ *Bulletin of the American Schools of Oriental Research*, 1948, loc. cit.

²⁵ *Iraq*, 1946; Rowton has to reject the usual Sothic dating which he avoids by taking 'the era of Menophres, to mean the era of Memphis and to have nothing to do with Seti I'; the latter assumption is probably true but the former is more doubtful.

Smith therefore proposed 1780-1730 as the probable limit of level VII correlating it with the rapid decline of Egyptian influence at the end of the reign of Amenemhat III but revised this after the discovery of seal impressions of Yarim Lim's reign showing the worship of an Egyptian deity. Woolley and Schaeffer therefore both think that the earlier levels of Atchana III may go back into the 19th century B.C. These levels contain a painted ware which Woolley described as Habur ware, a fabric well known from Professor Mallowan's excavations at Chagar Bazar and there dated 1900-1700 B.C. The Alalakh pottery is probably related to Habur ware but not identical with it. The fabric lasted a long time at Alalakh and in the 14th century produced patterns suggestive of Mycenaean influence, but there is no reason to suppose any Minoan influences of the earlier varieties. Indeed Woolley would rather suppose that the Cretan palaces and their frescoes were influenced by Syrian ones such as those of Alalakh and Mari. This theory would agree well with a late date for Hammurabi but the parallels between Crete and Atchana III are at present too vague to help us to an exact chronology of the 18th century.

For the late Minoan period a flood of light has been shed by the recent excavations at Pylos and Mycenae on the mainland and by the researches into linear Script B by various scholars including Sir John Myres, Dr Alice Kober, and Dr Bennett and culminating in the attempt by Messrs Ventris and Chadwick to read these texts into an archaic but quite intelligible form of Greek.

The absence of linear Script B tablets and of Late Minoan II pottery from Cretan sites except Knosos had always been a stumbling block to those who like Evans and Pendlebury had believed in the rule of a Cretan dynasty over Mycenae. We can now explain these by supposing that an Achaean dynasty may have ruled at Knosos during the L.M. II period while Minoan princes still using L.M. I pottery ruled in other parts of Crete and that the linear Script B was an Achaean modification of the Minoan linear Script A to meet the needs of a different language.

This evidence for the existence of an Achaean dynasty at Knosos before 1400 B.C. only affects absolute dating in so far as it supports the theory I have always held that the East Cretan sites were probably destroyed at the same time as the Palace of Minos and there is no need to postulate an earlier catastrophe about 1500 B.C., though I would not exclude such a possibility.

Till recently the known pottery of Late Minoan II style consisted chiefly of the large Palace amphorae supplemented by a small number of smaller vases such as the later vase from the temple tomb. In 1940 I excavated a small chamber tomb of this period containing a plated silver cup with gold rims, a bronze sword, a silver pin and some stone vases (one of Egyptian calcite) and pottery vases from one burial and more fragmentary remains of another.²⁶ More recently N. S. F. Hood and P. de Jong excavated a splendid series of warrior graves with engraved seals, weapons, a fine bronze helmet and M.L. II vases just north of the city of Knosos, and S. Alexiou excavated two other tombs at Katsamba behind the harbour town²⁷. Alexiou's tombs contained some fine vases which, he styled L.M. II and L.M. IIIa 1 and an Egyptian 'alabaster' jar (PLATE IX, and FIG. 1, p. 184) bearing the cartouche of Thothmes III²⁸. The L.M. IIB tomb which,

²⁶ In process of publication, but see *J.H.S.*, 1946, pp. 84, 85 and figs. 1, 2.

²⁷ *B.S.A.*, 1952, pp. 243-77, *Kretika Chronika*, 1952, p. 9 ff.

²⁸ *Kretika Chronika*, 1952, p. 9 and figs. 1 and 3; the stone described as 'alabaster' by the ancient Greeks would be termed calcite by a modern geologist.

MINOAN CHRONOLOGY REVIEWED

excavated near the temple tomb, also contained a jug of Egyptian 'alabaster' though in this instance the shape appears to be un-Egyptian.

Personally I incline to call all this group of tombs L.M. 11b because they seem too rich to be the immediate successors of the great catastrophe that overwhelmed Knosos about 1400 B.C. Alexiou, however, is unquestionably right in comparing some characteristics to Furumark's L.H. 111a 1 material. I would therefore suggest the following equation :

$$\text{L.M. 11b} = \text{L.H. 111a 1} = 1425-1400$$

$$\text{L.M. 111a} = \text{L.H. 111a 2} = 1400-1300$$

Typical of this L.M. 11b or L.M. 111a 1 period (in Furumark's series) is the squat alabastron, a form of vase which appears on the mainland with a cruciform design on the base in the L.M. 11 period and with concentric circles on the same place in the L.M. 111a period. L.M. 11b tombs (both mine and the Warrior Graves of Hood and de Jong) illustrate both methods of decorating the base. A squat alabastron of this kind from the tomb of the Macebearer at Zafer Papoura was described by Evans as a 'compendious illustration of the immediately succeeding ceramic style which may be referred to the early part of L.M. 111a. Chronologically even it may actually come within the limits of the Palace period²⁹' and so, I believe, it does.

It should be remarked that whereas much pottery of the L.M. 111a period looks like imported Mycenaean ware, these L.M. 11b vases, even if made in some instances for an Achaean aristocracy resident in Crete, were certainly produced by Minoan craftsmen to judge from their paint and fabric and decoration. One result of our realization that there was probably an Achaean dynasty in power at Knosos from 1450 B.C. is that Mylonas' objections to believing in the Athenian folk legends of their oppression by King Minos now lack substance³⁰. Mylonas is probably right in claiming that no prehistoric Cretan family ever ruled over Attica but there might well have been a short-lived oppression of that country by an Achaean King of Knosos without leaving any archaeological evidence for it. In other words we have a magnificent justification of the theory of the late Sir William Ridgeway that the Minos of the Athenian legends was no 'Minoan' but the Achaean destroyer of the Minoan Empire³¹.

For the neolithic period we have no exact synchronisms but Matz' dates seem reasonable and are reinforced by new datings for the Egyptian neolithic and Middle Predynastic cultures based on carbon 14 analyses, 4044 ± 205 B.C. for the Neolithic A stage in the Fayoum and 3205 ± 230 B.C. for a Middle Predynastic level at El Omari in Lower Egypt³².

With these guides I therefore submit the following not as a final chronology but at least as a reasonable emendation of my previous one. Evans' typology still holds good, but we are now in the unfortunate position of using his terms in two different senses. 'M.M. 1a' in the phrase 'M.M. 1a period', does not mean the same as 'M.M. 1a' in the phrase 'M.M. 1a style'. For the chronological periods we want something like the Early Knosian, Middle Knosian, etc., suggested long ago by Ridgeway but for the present I continue to employ Evans' terms in the time sense also only when I am discussing his periods. In the following table the terms refer to pottery styles.

²⁹ *P. of M.*, IV, p. 357, and Alexiou, loc. cit.

³⁰ 'Athens and Minoan Crete' in *Athenian Studies presented to William Scott Ferguson*, p. 7 (*Harvard studies in Classical Philology*, sup. Vol. 1).

³¹ *Proceedings of the British Academy*, Vol. IV, p. 1.

³² C. B. M. McBurney, *ANTIQUITY*, 101, March, p. 38.

ANTIQUITY

2900 ?	B.C.	Early Neolithic begins in Knosos				
2700 ?		Middle	"	"	"	"
2500 ?		Late	"	"	"	Crete generally
2400 ?		E.M. I	"	"	"	Northern and Eastern Crete
2300 ?		E.M. IIA	"	"	"	Eastern Crete and perhaps elsewhere
2200 ?		E.M. IIB	"	"	"	"
2100 ?		E.M. III	"	"	Eastern Crete	} sub-Neolithic wares linger at Knosos and Phaistos
1950		M.M. Ia	"	"	at Knosos	
1900		Mature M.M. Ia	"	"	in Central Crete, including Mesara	
1900		M.M. Ib	"	"	Eastern Crete	
1850		M.M. IIA	"	"	at Knosos and Phaistos	
1830		M.M. IIB	"	"	Knosos and perhaps elsewhere	
1700		M.M. IIIA	"	"	in Crete generally	
1600		M.M. IIIB	"	"	"	"
1550		L.M. Ia	"	"	"	"
1500		L.M. Ib	"	"	Eastern Crete	
1450		L.M. Ib	continues in Eastern Crete			
		L.M. Ia	"	"	elsewhere except at Knosos where L.M. IIA develops under the Achæan dynasty (? of the elder Minos)	
1425		L.M. IIB	begins at Knosos. Varieties of L.M. I continue elsewhere.			
1400		The great catastrophe affecting the whole of northern Crete : I am inclined to believe in Marinatos' theory of earthquakes followed by tidal waves but put it a hundred years later than he did, though there may have been two such events.				
1400		L.M. IIIA	begins in Crete generally			
1300		L.M. IIIB	"	"	"	"
1230		L.M. IIIC	"	"	"	"
1025		L.M. IIIC	ends.	"	Dorians from Argos colonise Knosos.	

When this whole was already in typescript I consulted my friend Professor H. W. Fairman on some doubtful points of the Egyptian chronology and received from him some valuable remarks of his own on Egyptian chronology and also some notes on a lecture on Mesopotamian chronology recently delivered in Liverpool by Dr Sidney Smith, and I should like to express my gratitude for these. Fortunately they confirmed what I had expressed on the more important points such as the dates of Hammurabi and the twelfth dynasty, but of course neither Professor Fairman nor Dr Smith are responsible for any statements in this article.

Notes and News

DESERT KITES

These stone-walled enclosures in the Syrian desert have long been a puzzle, now at last solved. On a cairn excavated by Mr Lankester Harding many of the stones had inscriptions, and one had a drawing as well (PLATE III, A, and FIG. 1); the inscription is translated as follows: 'By MANI'AT, and he built for HANI'. And he drew a picture of the pen (or, enclosure) and the animals pasturing by themselves'. The site of the cairn is described as 'about 15 kilometres east of the gate of the I.P.C. station of H5, and about 50 metres north of the main Baghdad road, map reading approximately 360.185'. The excavation of the cairn was first described and fully illustrated in the *Ill. London News*, October 10th, 1953, pp. 564-5, and we wish to thank Mr R. S. Newall for calling our attention to it. By permission of Mr Harding, we reprint his account from Vol. II of the *Annual of the Department of Antiquities of Jordan*, edited by him, pp. 30-1.

'The drawing accompanying this text is one of the most interesting yet found, for it shows us one of the enclosures hitherto known as "desert kites" in use, thus answering the two main problems connected with these structures, viz. what was their use, and their date. Attention was first drawn to these in *ANTIQUITY*, Vol. I, p. 202 and Pl. III, where a fairly accurate deduction as to their use was made by Flt. Lt. Maitland. The theme was further developed and illustrated by Group Capt. Rees in *ANTIQUITY*, Vol. III, pp. 395 ff. and Pls. III and IV. The most recent article is by Sir Alec Kirkbride in *JPOS*, Vol. xx, p. 1, and the present drawing shows that his is the most accurate suggestion.

On the left is the fortified enclosure in which a number of animals are already assembled: it has seven defensive embrasures in the walls and at the entrance the walls are turned inward to make escape of the animals more difficult. From the corners of the enclosure two long walls are shown extending; this is contrary to all known examples, where these long walls commence from the narrow entrance of the enclosure. The vertical strokes along these walls suggest that they were palisaded, and would account for the fact that they are always much lower than those of the actual enclosure. These palisades would presumably be made of branches of the desert trees and shrubs. On either side of the entrance, within the long walls, stands a man with arms upraised, directing the animals into the pen. At the mouth of the palisaded walls (which owing to the shape of the stone contract together instead of fanning out as they do in fact) are other animals being driven or stampeded towards the enclosure by a man with arms upraised. Outside the limit of the walls a few animals have succeeded in escaping and are making off in the opposite direction. The text commences here and continues over on the other face of the stone, where is another scene.

Here two quite different types of animals are depicted; the characteristics of one are a long tufted tail, a fairly long neck and small head, the ears being back. The other type has a long body, straight tail, a short neck and prick ears. There are three of each type. The former are being attacked by a man with a bow and arrow (or spear and shield?), while another with arms upraised seems to be trying to scare them off. The other three are all together in the right corner, and appear to be controlled by a man with a whip, or at least a long rope. They look almost as if they might be dogs. It is very difficult to guess at what the first group are supposed to represent: it is not the usual Safaitic

way of depicting a lion (for which see no. 78), but what other animals of the neighbourhood have tufted tails? It would seem that these are the animals which are menacing the flocks and causing them to be driven into the enclosure, so it must be some beast of prey. Perhaps the dogs are helping to hunt them down.

This is the first extended composition by a Safaitic artist that we have seen, and certainly the first to depict an episode in which a building is involved, so that we have as yet no knowledge of their artistic conventions. Comparison with the plan of an actual

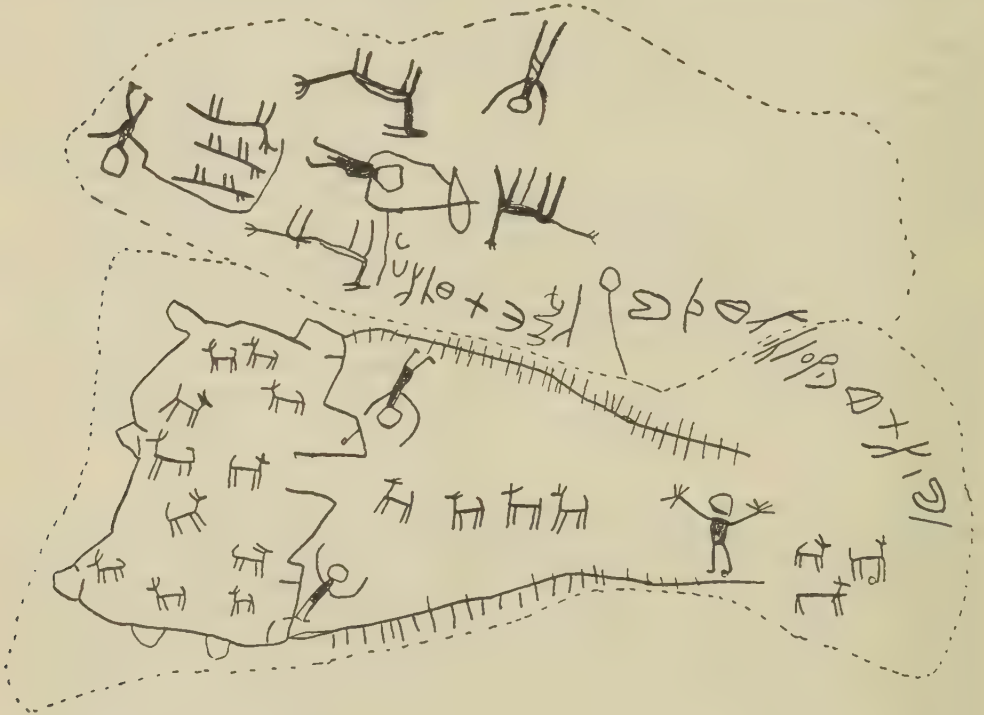


FIG. 1. EXPANDED COPY OF DRAWING OF 'KITE' (see PLATE III, A)

enclosure, Fig. 8 shows that apart from the variation mentioned above the drawing is a remarkably accurate representation. The other difference, the convergence of the long walls as against their fanning out in actual examples, is dictated by the shape of the stone on which the drawing is made.

It is, perhaps a little surprising to find that these grazing enclosures are a mere 1,500 or so years old, for they give the impression of much greater antiquity, but Rees (see above) put forward some arguments for a Roman date.

In the earlier part of this article Mr Harding says (p. 8):—

'The building of cairns over graves is in itself of some interest, and Hasan Awad pointed out that it is a custom still practised among the Bedu. But a cairn is only built over those who are killed, never over those who die a natural death, and the stones of which it is built must be brought from some distance away; those in the immediate

vicinity may not be used. Both these conditions are fulfilled in this case, as *Hani'* was apparently killed by an arrow, and the basalt stones and boulders which strew the desert surface still some right up to the cairn itself, a fact I had not appreciated until Hasan drew my attention to it. The reason for these customs is now forgotten, but probably has something to do with the placating of the disturbed spirit. It is also still the custom of passers-by to add a stone to the pile, as so many Safaites record doing.

Unfortunately nothing discovered in the course of excavation or in the texts themselves gives any satisfactory clue to the dating, though the Latin text No. 173 suggests a fairly early date, as Latin does not seem to have been used in Jordan after the 4th century A.D. But this particular text does not refer to *Hani'* by name, and might even be later than the burial. The last pilgrim to the cairn was a Polish soldier, who scratched his name on a block of basalt in 1943.

A REMARKABLE BOG-FIND (PLATES IV and v)

It is not often that one is able to look into the actual face of prehistoric man (PLATE v). In the summer of 1950 a corpse was found in Tollund Bog, about 10 kilometres west of Silkeborg in Jutland. It lay on the bottom of the bog, covered by 2.50 metres of peat. It was that of an adult male and was in an unusually good state of preservation, especially the head (PLATE IV). The internal organs too were intact, and their contents susceptible of examination.

On the head was a conical cap made of eight pieces of skin sewn together with the hairy side inwards, and tied under the chin with skin straps. Round the waist was a belt of an irregularly cut thong with an 'eye' in one end. The most remarkable feature about the body, however, was the well-plaited rope which, by means of a noose, was pulled so tightly round the man's neck that it had left grooves in the skin. The assumption is that the man was hanged by the rope.

The corpse cannot be dated either by the cap, the waist-belt or the rope. Nor is any clue provided by a preliminary analysis of pollen samples taken from round about the body. The relation of the cap to the skin cloaks mostly found on bog corpses, and the violent death by hanging nevertheless suggest that the Tollund body is yet another of a group of similar bog-corpses, now numbering about forty, that have been found, especially in northern Jutland. It seems plausible to assign them to the Early Iron Age, i.e. between the end of the Bronze Age and the year A.D. 400 approximately.

The foregoing is a summary, in English, printed at the end of Knud Thorvildsen's article, 'Aarbøger for Nordisk Oldkyndighed og Historie', 1950, p. 310. On pp. 311-28 is a botanical study in Danish of the stomach contents by Hans Helbæk, and on pp. 329-41 the same in English. The man's last meal seems to have contained no meat, and to have consisted of some kind of gruel, with mainly barley, linseed, *Camelina* seeds and fruits of *Polygonum lapathifolium*.

We wish to thank Dr Ramskou and the authorities of the Nationalmuseet, Copenhagen, for their kindness in supplying the photographs from which Plates IV and v were made, and for giving a reference to the published account used above. The discovery was the subject of a television programme recently broadcast and reported (and criticized) in the *Times*, June 6th.

THE KALAMBO FALLS PREHISTORIC SITE

A discovery likely to be of some importance in helping to determine the age, environment and ultimate development of the Chelles-Acheul Culture in Central Africa was made towards the end of last year at the Kalambo Falls on the Northern Rhodesia-Tanganyika

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territorial boundary, some 20 miles from the south-east corner of Lake Tanganyika. Here were found exposed in the deposits of a small Pleistocene lake five separate living floors giving for this region the stages of development out of the final Chelles-Acheul into the Middle Stone Age. Associated with the Chelles-Acheul horizons (in particular the lowest two) were found partly carbonized tree trunks which had been preserved in a water-logged state, since, it is believed, the Kanjeran-Gamblian interpluvial. The wood and cultural horizons lay on the edge of the old lake, which was drained probably at the maximum of the Gamblian, by the cutting back of the spillway at the gorge through the retaining hills. The wood should give a C14 date for the final Chelles-Acheul though it may well be that, until counting technique is developed further, a minimum date only will be possible. The wood is probably *Acacia*, while the clays in which it is usually found are rich in well preserved pollens. Examination of these has not yet been completed but the fairly high proportion of grass pollens (28%) Compositae, and Chenopodiaceae pollens confirm that the final Chelles-Acheul was associated with a fairly dry climate and open environment. Unfortunately all vestige of bone has disappeared, destroyed it is believed, by the peaty acids in the deposits, but the associated fauna is now fairly well known from the East African sites.

This is the first time so far as is known that wood has been found preserved with handaxes in Africa and the association of this with actual camping floors holds out high promise that wooden tools may be found when further excavations are undertaken, although at this site two pointed fragments of wood have already been found which may possibly have been utilized. The implements in the floors are very prolific and the material used, chert, quartzite and fine-grained sandstone, enabled many finely made tools to be produced. As yet only preliminary excavations have been carried out but they are sufficient to hold out the promise of important discoveries in the future. [For a fuller account see *The South African Arch. Bulletin*, vol. IX, no. 34 (June 1954). Ed.]

J. DESMOND CLARK.

CELTIC FIELDS ON THE LONG MYND

In 1938 Group-Captain Livock discovered and photographed a fine group of lynchets on the southern slopes of the Long Mynd in Shropshire. Those here reproduced are from enlarged prints of copies made from smaller prints kindly lent by Group-Captain Livock. They are almost but not quite the first to be recorded in this region, the first being those discovered and excavated by Mr B. H. St. J. O'Neil, now Chief Inspector of Ancient Monuments, on the Breiddin, Montgomeryshire.*

Last Spring I surveyed the area, using some R.A.F. photographs as a map on which to insert the banks, which they did not show. I had particularly hoped that it might be possible to discover the relations between a round earthwork in the foreground of Group-Captain Livock's obliques and the field-banks. Several banks impinge upon it ending abruptly there as if they had been cut through by the makers of the round enclosure.

One of the photographs (PLATE VI) is a distant view covering most of the top of the Long Mynd and extending northeastwards beyond to the pass of Church Stretton, with the hill of Caer Caradoc dimly seen in the far distance. At the bottom is a small white house close to the site of a station on the long derelict Bishop's Castle railway line. Beyond the wood on both plates is a tangle of interlacing traffic-ruts of all ages; one at least is contemporary with the fields for it can be followed on until it runs into a well-defined double-lynchet way leading down to the coombe on the north. Others are of medieval origin—bifurcations of the Portway, the road leading to the town of Shrewsbury.

* Described in *Archaeologia Cambrensis*, XCII, 1937, 89-91, 112-113.

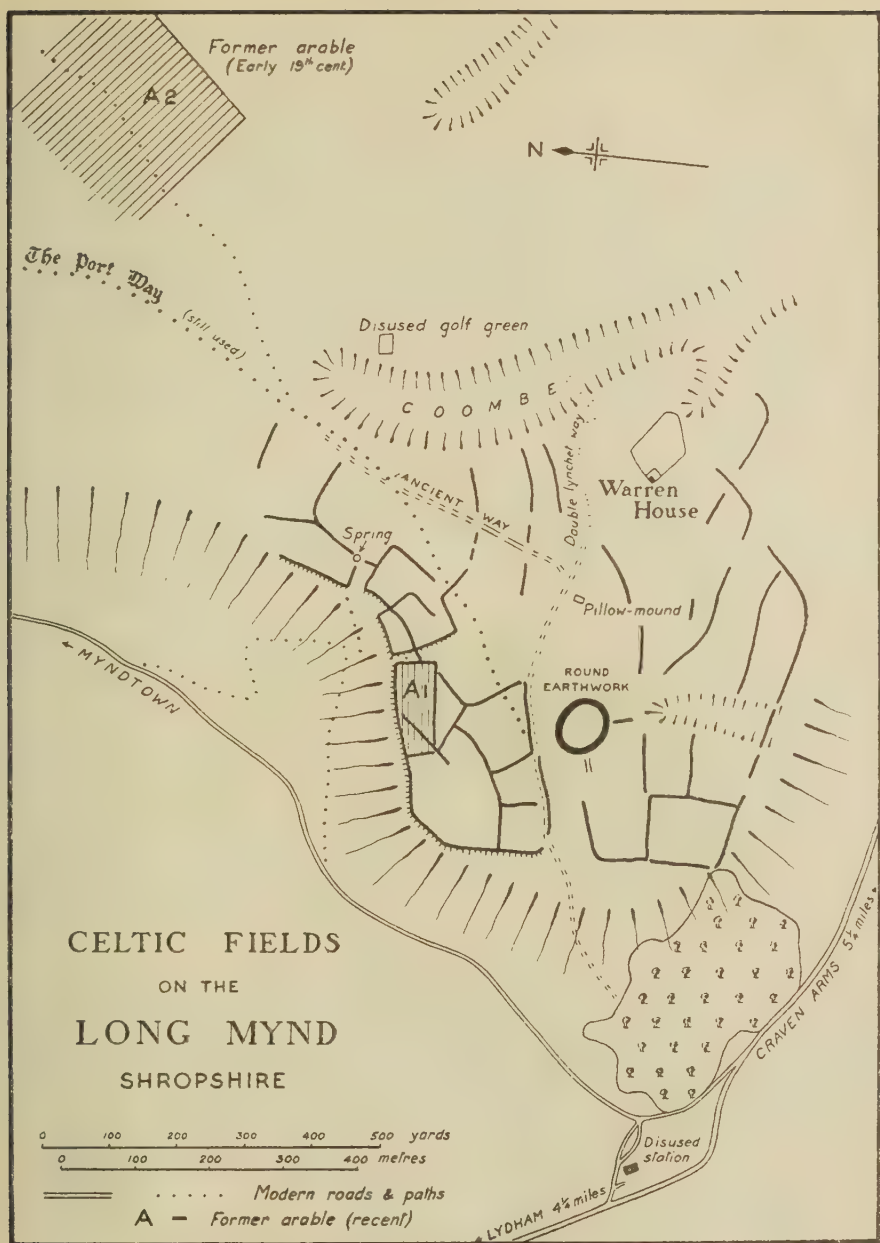


FIG. 1.

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(The O.E. *port* meant town, often market town). The lynchets themselves seem to aim at, but not always achieve, a chequer pattern of square fields. There is a big and well developed lynchet round the outside of the cultivated area, broken by a gap in the middle where a small stream and its valley break into the edge of the plateau. I do not know what to make of the round earthwork, but it was obviously not made for defence—in any military sense, at any rate; the bank is so low as to be hardly perceptible on the ground, and the ditch is almost completely silted up except on the north side. There are no signs of habitation within the circle, and neither here nor anywhere else in the area could I find any potsherds in the mole-hills.

At A1 on the diagram is a narrow rectangular field bounded by small sharply defined banks; the air-photograph (PLATE VII) reveals a series of parallel plough-ridges which are only faintly perceptible on the ground and would never be observed if not thus previously brought to notice. At the NE corner of this old field are the ruins of a small building. The field and its cultivation probably date from the Napoleonic wars; there still lingers in Church Stretton the dim memory of cornfields on the plateau, of which those at A2 are also part.

The area is marked 'Rabbit Warren' on the Ordnance Maps, and Warren House, at a lovely spot on the eastern slope, was no doubt where the warrener used to live. Midway between it and the round earthwork is a fine pillow-mound, a type of earthwork now proved to have been constructed for the use, and convenient trapping, of rabbits.

The date of these ancient fields can only be proved by excavation, but it would be a safe guess that it is the same as that of Mr O'Neil's at the Breiddin, that is to say, his Period III, covering most of the Roman occupation (*circa* A.D. 75–380). At both sites there is evidence of field-walls and of field-ways. I observed from a distance what looked like the lynchets of a similar system on Hope Bowdler Hill and others (less clearly) on the lower ground north and east of Caer Caradoc.

Excavation might profitably be undertaken to determine the relative ages of the round earthwork and the banks impinging on it. Trenches through some of the bigger lynchets might reveal interesting remains of field-walls. Another might be dug across the double-lynchet way.

It is greatly to be desired that this interesting area should be scheduled and kept as an open grass pasture-ground, which it now is. It would be particularly distressing if it were to be afforested and thereby closed for ever to ramblers interested in field-archaeology.

O.G.S.C.

PAN-AFRICAN CONGRESS ON PREHISTORY

We are asked to state that the Third Congress will be held in Livingstone, Northern Rhodesia, in July, 1955, probably between Monday 11th and Saturday 16th of July. During the course of the session, delegates will have the opportunity of visiting the Victoria Falls (six miles distant from Livingstone) and the more important prehistoric sites in the Zambezi and tributary valleys in the vicinity of Livingstone.

The business of the third session will be partly devoted to the reading of papers and to discussions upon discoveries that have been made since the second session. It is proposed that symposia shall be held on such subjects as—

The Kalahari Sands, their definition, age, distribution and method of formation.

The dating of the Australopithecinae.

Marine Strand Lines, their definition, altimetric and faunal correlations, being reports by the Commission for the Study of Strand Lines set up by the fourth I.N.Q.U.A. Congress.

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The early pebble tool cultures, their definition, dating and distribution.

The importance and application of the C14 method of dating to African prehistoric cultures.

It is also proposed to hold discussions on terminology, research programmes in prehistory and allied subjects in the various African territories, the control of excavations and the care and protection of sites, and the preparation of resolutions on these subjects.

After the closing plenary session and before the meeting, if circumstances so dictate, delegates will have the opportunity of participating in excursions to the main pre- and proto-historic sites in the Rhodesias in so far as time and distance permit. These excursions, which are aimed at giving delegates an opportunity of seeing the principal type sections upon which the Quarternary climatic and prehistoric successions have been built up, will include visits to such sites as the Bembesi River and Chelmer near Bulawayo; rock paintings in the Matopos Hills and near Salisbury; Zimbabwe near Fort Victoria and Khami ruins near Bulawayo; one or more of the Lusaka fissure deposits; Broken Hill; pebble culture sites at Kalomo, and a visit to the Wankie Game Reserve. It is hoped also that it may prove possible for an excursion to be arranged to visit some of the more important southern Katanga sites in the Belgian Congo. Delegates would travel in sedan cars.

Those who are interested and think of attending should write for the preliminary circular to:—J. Desmond Clark, Organising Secretary, P.O. Box 124, Livingstone, Northern Rhodesia.

ARCHAEOLOGY AS A CAREER

We are also asked to state that, in the winter number of *Archaeology*, published by the Archaeological Institute of America, there will be printed an article on 'Archaeology as a Career', written by Professor John H. Rowe, Associate Professor of Anthropology in the University of California. In this article Professor Rowe describes in detail the education and preparatory work desirable for a young man or woman considering an archaeological career. He also describes the various kinds of posts available and the rewards, financial and otherwise, which may be expected. The article will be illustrated and includes a brief bibliography of useful reading material.

In the belief that such an article will be of wide interest it is planned to print separate reprints which can be obtained either singly or in bulk. It would be of great assistance if those institutions or individuals who wish to order 50 copies or more would place their orders in advance of publication. After publication it may not be possible to fill large orders. Such orders should be addressed to the Editor, 'Archaeology', 211 Jesse Hall, University of Missouri, Columbia, Missouri. The reprints will be sold at cost price, \$3 for 50 copies, \$5 for 100 copies, postage extra. Requests for fewer than 50 copies should be addressed, after January 1, 1955, to The Business Manager, 'Archaeology', Andover Hall, Cambridge, 38, Massachusetts.

Reviews

PRESIDENTIAL ADDRESS : All-India Oriental Conference, Archaeology Section, Ahmedabad, 1953. By A. GHOSH, *Director General of Archaeology in India*.

FURTHER COPPER HOARDS FROM THE GANGETIC BASIN. By B. B. LAL. *Ancient India*, no. 7 (1951), 20-39, 8 figs., 7 pls.

The Archaeological Survey of India is continuing its campaign to elucidate the Dark Ages between Indian prehistory and history by the techniques of field archaeology. In the two papers under review, significant advances have been recorded beyond the position summarized by the reviewer in *ANTIQUITY* XXV (1951), 166, and the main points are given below.

Mr Lal's recognition of the importance of Painted Grey Ware in the Upper Ganges region, where he was able to demonstrate its association with sites traditionally connected with events in the *Mahabharata* epic, and to show that it antedated the Northern Black Polished Ware belonging to the second half of the first millennium B.C., has been taken a stage further. New excavations at the Harappa site of Rupar, 60 miles north of Ambala, have shown that the Harappa phase is overlaid by one with Painted Grey Ware, followed by early historical occupations from Mauryan to Gupta times. More evidence from other sites is obviously to be desired, but the implication that Painted Grey Ware represents a pottery style of an immediately post-Harappa culture in north-western India is an attractive one. Technically, the pottery in question could be related to that from such Makran sites as Khurab and Shahi-Tump, with an inferential date around or after c. 2000 B.C., though it would be difficult to bring down the chronology of these sites by a thousand years, and so within the range of time likely on available evidence to contain the Painted Grey Ware culture in India. But the implications of the *Mahabharata* associations are of course with the Aryans, and both Khurab and Shahi-Tump provide evidence suggestive of invasive movements from the west. The entry of the speakers of Indo-European languages into India need not have been a unitary or a simple process, and we may in time be able to recognize a series of related movements of migration and invasion in the archaeological evidence, spread over a millennium or so.

The copper hoards from the Gangetic Basin were discussed by the reviewer in *ANTIQUITY* XVIII (1944), 173. Mr Lal not only publishes, in admirable detail, a mass of new material, but convincingly shows that previous conclusions on these curious assemblages of copper tools are likely to be erroneous. The reviewer, with others, confused the characteristic composition of the hoards (flat and shouldered axes, harpoons, 'bar-celts' and 'anthropomorphic' figures of sheet copper) with other finds of swords, shaft-hole and trunnion-axes, and tried to glimpse an Aryan ghost behind them all. Mr Lal makes a much better case for supposing the objects from the hoards to represent a native culture more or less confined to the Gangetic Basin and complementary to that of Harappa further to the west. In two sites a not absolutely convincing association can be made between copper hoards and sherds of thick ochre-washed pottery; similar sherds occurred in the earliest level at Hastinapura, overlaid by the Painted Grey Ware stratum. It is to be hoped that more direct evidence for association and date will be forthcoming.

With the recognition of distinctive pottery types such Painted Grey Ware and Northern Black Polished Ware, a relative chronology of stratified sites in northern India becomes possible. Mr Ghosh points out that the latter pottery has now been found as

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far east as Bengal and as far south as Hyderabad, and that its general association with the Mauryan Empire is becoming more and more likely. In Central India, in sites such as Tripuri and Nasik, it is possible to show that a culture characterized by a microlithic flint industry and painted red pottery flourished at a period before the appearance of Northern Black Polished Ware in the region, and the great fortified site of Old Rajgir can now be associated with the same black pottery. In the Mauryan period, new excavations at Pataliputra (cf. *Ancient India* no. 4 (1947-8), 96-108 for earlier work) apparently cast doubt on the existence of the famous pillared hall with polished sandstone columns, but surely nobody but Spooner ever believed that the 'missing' columns had really sunk vertically into the soft subsoil until they were out of sight? STUART PIGGOTT.

THE EARLY CHARTERS OF DEVON AND CORNWALL. By H. P. R. FINBERG. *Department of Local History, University College of Leicester, 31 pages. Price, 6s.*

It is a frequent complaint of medieval historians, in England and abroad, that no complete, scientific, edition of Anglo-Saxon Charters has yet been prepared. A number of specialized editions, particularly of writs, wills and vernacular charters have appeared, but no major operation has yet been planned to advance the work of J. M. Kemble's *Codex Diplomaticus* (1839-48) and W. de G. Birch's *Cartularium Saxonicum* (1885-93). Now Mr Finberg is throwing the local historian into the breach. 'Can we not parcel out the field', he asks, 'and attack the problem on a geographical basis'? He issues as a piece of 'preliminary spade work' a calendar of 104 documents (71 for Devon, 33 for Cornwall) giving the probable date, the names of parties and places concerned, and a reference where the charter may be found (or, in the case of charters now lost, where mention of it is made). Further, he throws out by way of bait a taste of the kind of treasure which may still be concealed in the material. In a pretty piece of reconstruction he turns up a Cornu-British pocket in 8th century Devonshire which, he suggests, was not disturbed before the reign of Athelstan. Whatever finds may await the local historian and whatever his contribution to the identification of place-names may be, ideally the task of editing should surely belong to the student of diplomatic working from the centre, who can survey the whole material and set up a standard of dating and authenticity. But if such a man, or group of men, cannot be found all honour and success to Mr Finberg's scheme. E. O. BLAKE.

POTSHERDS. By HAROLD S. COLTON (*Northern Arizona Society of Science and Art, Flagstaff, Arizona, January 1953*). 18 figs. (mainly photographs). \$3.

This 'expands and replaces' the first chapters of Colton's *Handbook of Northern Arizona Pottery Wares*, the author's intention being to revise the whole for republication in separate parts. The foreword tells us that 'a need has arisen for a more general work on how to study primitive pottery, without necessarily stressing the problems of a local area', a need which, presumably, the present work is meant to satisfy. Its subtitle gives a better definition to its intended scope: *An Introduction to the Study of Prehistoric Southwestern Ceramics and Their Use in Historic Reconstruction*. But it must be said that the 'use in historic reconstruction' (whatever that may be) of this book is questionable; and it is never clear for whom the introduction is intended. Elementary statements (e.g., 'the most important single ingredient of pottery is clay') jostle with electric combustion furnaces, rheostats, galvanometers and dynamometers. The strength of pottery, we are told, is measured by a dynamometer which records the force required to break a piece 'of standard size', but the application of this and other items of gadgetry is hardly

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mentioned, let alone justified. Some of the implications are quite frightening ; one feels a shadowy prehistoric Inspector of Ceramics looming in the background, brandishing a blood-stained dynamometer and hung about with other items of Mr Colton's formidable 'minimum apparatus needed for the study of potsherds'.

In this book classification for classification's sake runs riot, and the author is clearly determined to make of archaeology a Science with a very large capital S, by measuring in every possible way everything within reach. How much wiser he is after making all his computations is not clear. Perhaps the next volume will tell us.

Chapter II, describing and illustrating with photographs the procedure of a Hopi potter, is the most straightforward part of the book. Its four-and-a-bit pages, plus a few scattered data, in Chapter III mostly, are all that will be useful to European students, if they happen not to have seen it all before in one place or another. For most of us *Potsherds* will be merely a vastly complicated thrashing of a very dead horse.

BRIAN HOPE-TAYLOR.

LAGORE CRANNOG: AN IRISH RESIDENCE OF THE 7TH TO 10TH CENTURIES. By HUGH HENCKEN, D.LITT., M.R.I.A., F.S.A. *Proceedings Royal Irish Academy*, vol. LIII, Section C, No. 1. (Dublin: Hodges, Figgis and Co.) 19 plates (incl. 4 folders), 117 figs. Price, 25s.

This volume embodies the results of the excavations of the Harvard Archaeological Expedition in Ireland, 1934-6, on this classic site (six miles northwest of Tara, in County Meath).

To say that its outstanding contribution is its splendid *catalogue raisonné* of the finds is not to slight it as a whole. Nineteenth century depredations caused confusion, and, in some parts of the site, total loss, of the structures on the crannog (the objects found at those times are as fully described and illustrated as the new material, incidentally). Moreover, persistent flooding prevented full excavation of some areas, which are somewhat vaguely indicated on the main plan. To a very great extent these factors account for the rather inconclusive results obtained from the area contained by the three palisades, and, in any case, the inherent difficulties of crannog excavation are such as to invite sympathy rather than criticism. Nevertheless, the photographs suggest that larger-scale plans of various areas and horizons might have been helpful, and one cannot but regret that the siting of the published sections was such as to give practically no information about those portions of the site where it seems most likely that dwellings existed.

In passing, one notes the descriptions of the cellular structures encountered by Wilde and Wakefield, two of the 19th century excavators of Lagore. The reviewer would like to point out the striking correspondence between these accounts and the structures of the fortified Iron Age lake-side site at Biskupin, Poland¹. Remote in every way as the connections are, they may tend to give credence to the statements of Wilde and Wakefield. Other, perhaps confirmatory, details come from the recent excavations. The slotted posts at Lagore are exactly paralleled at Biskupin. Corner-posts with vertical slots at right angles are common to both sites (and also occur in 11th-13th century contexts in England and Denmark²). Other common elements, widespread in space and

¹ ANTIQUITY, XII (1938) pp. 311-17, 4 plates, 1 fig. *Archeologické rozhledy*: Ročník, v, 1953, Sešit 4, pp. 489-512: some duplication with above, but contains additional photographs, plans and reconstruction drawings and covers post-war work on this site. There is a summary in French.

² *J.R.A.I.*, CVII, 1952, pp. 15-43.

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time, point these similarities. Probably these were merely independent solutions to a common problem, but can we yet say that there was not a pervasive basic tradition of timber building which died hard?

It is not easy to justify the lack of correlation between the finds and the plan and sections. The author has prefixed the description of each object *Period Ia, Ib, I, II, III*, or *unstratified*, but this is hardly adequate. One needs to see at least some of the objects in their horizontal and vertical contexts. Might not the various horizontal distributions have given clues, however slight, to the uses of different parts of the site?

The catalogue of finds, on the other hand, leaves nothing to be desired and is most impressive, comprising a great diversity of objects in bronze (some with millefiori enamel), iron, pottery, glass, amber, wood, leather and bone. For photographic details of the important engraved bones, the reviewer would cheerfully have sacrificed Plate XI, fig. 2, which is below standard and retouched! The bronzes, a wooden figure and a dug-out boat are of particular interest, but the whole range of finds is outstandingly valuable. Many of the drawings are excellent, and all are adequate. The discussions of individual pieces are as comprehensive as space and relative importance warrant. Dr Hencken deserves our thanks for his treatment of this invaluable material.

There is an exhaustive section on the history of Lagore, from the Annals and other sources, by Liam Price, M.R.I.A., and the textiles are dealt with by Laura E. Start, M.A. The human and animal bones, and the evidence of bronze-, iron- and glass-working on the site, are treated separately. Those outside Ireland will find the bibliography particularly useful.

In the absence of an index (presumably not the author's fault), a table of contents and a list of figures and plates, or cross-references between plates and text, would have assisted the reader; but to some extent the orderly grouping of objects according to material saves one from confusion.

This is a massively informative work, indispensable to all students in its field.

BRIAN HOPE-TAYLOR.

BOATS AND BOATMEN. By T. C. LETHBRIDGE. '*The Past in the Present*' series, Thames and Hudson, 1952. pp. 199, 41 drawings by author, 4 plates. 15s.

Mr Lethbridge is probably our most conscientious archaeological buccaneer, constantly bringing us startling trophies of his raids on territories familiar and obscure. He has discovered islands of truth unnoticed by those who sail the usual channels, and has peppered our charts with archipelagoes of conjecture. Full exploration and colonization he leaves to any who follow in his wake—a wake which has rocked many antiquarian boats at lazy anchor! Even on those occasions when, despite much skilful sailing against the wind, he fails to bring us to the landfall of logical conviction, he usually succeeds in giving us an exciting run for our money, and leaves us dissatisfied with the repetitive 'All aboard the Skylark—once around the harbour' trips to which we are all otherwise too accustomed. Sometimes, without proof, he gives us an uneasy thrill of belief akin to that inspired in landlubbers by an old salt's tales of sea-serpents. And few surely, can withhold admiration entirely when he fires broadsides at what he believes to be derelict hulks of fallacy blocking the roads to archaeological harbours?

That *Boats and Boatmen* should have come from the pen of this sailor in deep waters and shallow is just as it should be. Mr Lethbridge, as others have remarked, is never dull, and he has made this book as lively, controversial and informative as one would expect. The author has had a wide experience of sea- and river-going craft, and draws on this greatly to the benefit of those who have not. The breezy explanations of various

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builds and rigs are particularly lucid and useful. Mr Lethbridge himself warns us, however, that many of his general conclusions on the evolution of different kinds of boats are heretical (notably his derivation of certain surviving types from Roman forms). It is amusing to reflect that he is here widening the exercise of his peculiar and enlivening gifts, that subscribers to ANTIQUITY may find themselves in the same boat as habitual readers of the *Mariner's Mirror*, subject alike to the sudden, revealing lighting-flashes of intuition and to the precipitating clouds of ingenious surmise from which they strike. If it is the author's aim, as one might suspect, so to stimulate (and occasionally, perhaps, so to enrage) both these unfortunately insulated groups that they seek each other's counsel, one must look forward to its fulfilment.

The section contrasting the psychologies of seamen and landmen, extremely relevant to the theme of the book, is an attempt to relate and balance the technological with the purely human aspects. Emphasis on the conservatism of the boat *builder* is a point of interest. Would that the effects of trades, crafts and environments on human temperament and social behaviour received more notice from archaeologists in general. (For example, the implications of the frequent near-insulation of medieval potters from more normal social groups by their need to settle where clay, sand, timber and water lay together, have so far received little attention).

Mr Lethbridge, as one would expect and hope, deals at length with the sailor's superstitions and rituals. He has a great deal of fun with the oculus and the ship's bell, sacrifices to the sea gods, figure-heads, boat-burials, the dangers of carrying parsons aboard, etc.

It will be sad and surprising if this book does not stir up interest in the important problems of early water transport, neglect of which must prejudice understanding of the trade and migration dependent on it.

BRIAN HOPE-TAYLOR.

AGRICOLA AND ROMAN BRITAIN. By A. R. BURN. *London, English Universities Press Ltd., 1953. 7s 6d.*

Mr Burn is to be congratulated on his latest contribution to the 'Teach Yourself History' series. He has provided an informed biography of Agricola, with special and often highly felicitous reference to the contemporary background. Special attention may be directed to his realistic treatment of the campaigns of Suetonius Paulinus, with a convincing location of the decisive battle against Boudicca not far from Towcester (though I fancy that his chronology of this governorship is too streamlined). A brief review cannot do full justice to a book which specialists, no less than the general reader, will find exceptionally stimulating.

ERIC BIRLEY.

INVENTORIA ARCHAEOLOGICA: Corpus des ensembles archéologiques, publié sous la direction de M. E. MARIEN. *Âges des Métaux Belgique, fasc. 1 (fiches BI-B10) par M. E. MARIEN. Published for the Congrès Int. des Sciences pre- et protohistoriques by De Sikkel, La Morinierstraat 116, Anvers, Belgique. Price, 90 Belgian francs. 1953 (received February, 1954).*

We extend a most hearty welcome to this, the first brochure of a series that every student of the European Bronze and Iron Ages will eagerly acquire. The purpose is to present in an accessible form drawings of *associated objects*, whether found in graves or hoards; for, as Dr Marien the originator says, these are of capital importance for the chronological scheme, and are often buried in rare local publications and museums. It was to meet a similar need that Harold Peake and the reviewer initiated the British

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Association (Section H) Card Catalogue of Bronze Implements ; but valuable though that catalogue still is publication was not envisaged. The need of some such plates as those of this Corpus is felt whenever one has to read an article on the Bronze Age in which the evolution or distribution of types of objects are discussed.

The objects drawn on these Plates include examples of the usual things—tools, weapons, containers and minor accessories ; all are published big enough to show details—a most important point. It would be nice to see some of the French hoards such as Treboul made accessible in this excellent format instead of the minute scruffy drawings usually thought good enough in that country. Some of the bigger hoards, e.g. Venat's Cascina Ranza, may be difficult to deal with, but this has already been foreseen and provided for.

The *Inventoria Archaeologica*, abbreviated for bibliographical purposes as *IA*, should be supported by all museums and libraries concerned with prehistory. O.G.S.C.

AN ANGLO-SAXON CEMETERY AT PETERSFINGER, NEAR SALISBURY, WILTS., by E. T. LEEDS and H. DE S. SHORTT, published by the Salisbury Museum, 1953, pp. 64, 11 plates and 15 figs., including two site-maps and a plan of the cemetery. Paper bound, 7s 6d, postage 6d.

Almost annually since at least 1912 Mr Leeds has made some contribution in the form of book, paper or review, to the study of Anglo-Saxon archaeology. Indeed, in both the quantity and value of his contribution he stands unrivalled. This present work, in collaboration with Mr H. de S. Shortt is a worthy addition to the series. It includes technical appendices on the construction of the weapons by Mr R. J. C. Atkinson, on the textile remains by Mrs G. M. Crowfoot and on the skeletal remains by Professor A. J. E. Cave.

The Peter's Finger cemetery lies near the ridgeway which crossed the Avon at Britford and afforded communication between the early English settlements of the Ebble valley and those east of the Avon. Thus were linked the villages represented by the small graveyards on Winkelbury Hill (Berwick St. John) and at Broad Chalke, and possibly by the isolated burials above Alvediston, Ebbesborne Wake and Coombe Bissett (Salisbury Race Course). The important Harnham Hill cemetery is but half a mile north of the track and not two miles direct from Peter's Finger. To the east of the Avon a single grave was found at Standlynch and a small cemetery on Roche Court Down, Winterslow. But the importance of this new cemetery consists in its virtual repetition of the cultural sequence in the Harnham Hill graveyard : an earlier, Jutish, stratum having affinities with the material common to 6th-century Kent, the Isle of Wight and southern Hampshire, all of which, according to traditions preserved by Bede, were settled by Jutes ; and a probably later stratum with Saxon affinities (p. 5). The Jutish material is exemplified by a small semi-circular-headed brooch (xxv, 79) similar to others from Chessell Down in Wight, Bifrons and Faversham in Kent, and Harnham Hill. Its occurrence elsewhere in England is a matter of no small significance and it would have been useful if the authors had specified the few other find-sites referred to on p. 45.

The jewelled buckle-plate (xx, 55) is one of a widely distributed type found in all the south-eastern counties and as far afield as Suffolk and Gloucestershire. One was recovered from a cemetery at Fox Hill, Wanborough, in northern Wiltshire and is now in the Devizes Museum. Sometimes regarded as exports from Kent, where they have occurred in at least six places, a pair was also found in the cemetery at Herpes (Dép. Charente) across the Channel. This fact gains in importance from the occurrence there also of a brooch closely similar to that just mentioned from Peter's Finger (xxv, 79)

(plate xiv, 2, *British Museum Guide to Anglo-Saxon Antiquities* (1923)). But it should be noted that this and other objects, common to Herpes and Jutish cemeteries in England, are exceptional in the French graveyard, which is otherwise purely Frankish in its affinities; and it is inaccurate to describe the common cultural element as Frankish (as on p. 4 of the work under review). In view of the distribution of similar objects in the Jutish regions of England, the element can only be termed Jutish wherever it is found.

Of similar date, the mid-6th century (p. 48), is the uncommon type of buckle from grave LI (find 144). This, too, has parallels in Wight and Kent (p. 50). Yet another connection with Wight, and with Harnham Hill, is to be seen in the bronze buckle from grave LXIII A (find 177). Besides these affinities with objects from Jutish England and from Herpes, there are others, Anglian (p. 51), Saxon, and more purely Frankish (pp. 53-4), such as may be observed among the grave furniture of a number of cemeteries in eastern and southern Britain. The Frankish element in Anglo-Saxon material culture is most marked in Jutish regions, no doubt as a result of permeation which preceded the marriage of Aethelberht of Kent with the Frankish princess Bertha some time before 588. This permeation was made the easier by geographical proximity. Yet even the earliest material from Kentish graves (excluding the Saxo-Frisian element common to Kent, Norfolk, Lincolnshire and Yorkshire) has more in common with Frankish than with Anglian or Saxon culture; and there is much to be said for the hypothesis that Bede's Jutes had sojourned for a while, during their migration from Denmark, among the Franks of the middle Rhine. They finally settled in Kent in the later fifth century where already before them Saxons from the Frisian coast had made their homes. [If 'Jutish' means anything at all, it cannot be applied to the Saxo-Frisian settlement in Kent, as it is by Mr Leeds (p. 3). The earliest phase of Jutish culture there is represented by grave-groups such as he illustrates from Chatham Lines (*Archaeology of the Anglo-Saxon Settlements*, fig. 20 or Jessup, *Anglo-Saxon Jewellery*, plate vi). None of these objects would be alien to the Isle of Wight, the only other of Bede's Jutish regions which has yielded adequate material for comparison]. The Saxo-Frisians of Kent find no mention in the historical records unless we identify them with the Hengist and Horsa episodes of Bede and the Chronicle, and see in the reinforcements mentioned by Gildas (and, following him, Bede), the Jutes who became culturally dominant in Kent. At any rate, the Saxo-Frisian element was ousted or submerged, and the Jutes, through trade, facilitated by political ties, continued to receive augmentation of the Frankish strain in their culture. It seems unnecessary, however, to postulate actual Frankish immigration to explain this strain as does Mr Leeds (*Early Anglo-Saxon Art and Archaeology* (1936), p. 57).

All this may be reasonably assumed from the character of the ample quantity of grave furniture from Kent. From Wight and to a small extent from the Meon valley in Hampshire, the same sequence of cultural trends is less apparent, largely no doubt because of the paucity of the material evidence. Yet it is, even so, enough to reveal the same sequence of affinities.

It now appears more certain than ever that southern Wiltshire, as well as Wight and Hampshire, received an influx of people who on cultural grounds can only be equated with Bede's Jutes. Their relics, and those of later Saxon origin have been described in some detail elsewhere (See my *Conquest of Wessex*, p. 122 f.). The material evidence from Peter's Finger reinforces the argument for an extension of Jutish settlement from the South Coast, and a subsequent mingling with the Saxon culture of the Thames valley and north Wiltshire, presumably after the unification by Cynric and Ceawlin of the Saxon and Jutish elements of Wessex. The Peter's Finger cemetery includes a slighter Saxon element (pp. 4-5)—the saucer-brooches (xix, 43 and 44) and the bronze disc (xlxb, 134)—

than was recovered at Harnham, though the number of graves was almost the same. The later, Frankish, element in southern Wiltshire is more clearly represented : in the Coombe Bissett jewellery and in that from Roundway Down (p. 4).

According to our only historical source, a band pushing northward from the South Coast reached Charford in 519 and a generation later, in 552, they defeated the British at Old Sarum. It is interesting to record that Mr Leeds now accepts the Anglo-Saxon Chronicle as a reliable basis (p. 3), and that he would term the comitatus of Cynric 'Juto-Frankish'. Preferable is the suggestion put forward by Mr G. M. Young, that the people later known as the 'Gewissas' were in fact an offshoot of the Saxones Eucii (amended to Eutii) mentioned in a letter of the second half of the 6th century as a tribe subject to Frankish overlordship. (*The Origin of the West Saxon Kingdom* (1934), p. 32; see also H. M. Chadwick, *Origin of the English Nation* (1924), p. 91 f., 100 f.). In this tribal group we have a Jutish and Saxon strain (according to one interpretation of the compound name) and close connection with the Franks; but if those buried at Peter's Finger and Harnham were indeed Saxones Eutii, then we should have to assume contemporaneity of Jutish and Saxon culture from the beginning, whereas the grave-goods suggest transition from the one to the other.

Altogether, this publication is fully worthy of the importance of the discoveries it records. Well illustrated and skilfully described and discussed in the text, the objects from Peter's Finger provide valuable new evidence for the elucidation of the English settlement; yet, it must be regretfully added, the work would have gained in usefulness, short as it is, if an index had been provided. A minor error on plate ix confuses 173, LXIIIa (key) with 173, LXIIIe (iron boss), for the latter is wrongly labelled. Another occurs on p. 4, where 'Charante' should be 'Charente'. Finally, a line-drawing of the ornament on the bronze plate of xix, 46 would have been more interesting than some that are provided.

GORDON J. COPLEY.

THE CAMBRIDGE ECONOMIC HISTORY OF EUROPE, VOLUME II. Trade and Industry in the Middle Ages. Edited by M. M. POSTAN and E. E. RICH. Cambridge University Press, 1952. 604 pages, 4 plates of illustrations, 11 maps. Price, 45s.

Much has changed since the Cambridge Economic History was planned. The original team of general editors, J. H. Clapham and Eileen Power, who inaugurated the series with a wholly admirable volume on Agrarian Life in the Middle Ages, have been succeeded by Professors M. M. Postan and H. J. Habakkuk. The war, and the resulting delay, as it had left its mark on the pages of the first volume, has not allowed the plan of the second to escape unscathed. Its scope which was to be 'urban, industrial, and commercial' has been restricted to trade and industry only. Separate chapters projected on Trade in Eastern Europe, Shipping, and Transport had to be omitted, and the burden distributed to increase the load of other contributors. On the other side of the scales chapters outside the professed range of the Middle Ages and of Europe and topics on price movement and population have been added. We are left with two central essays on the trade of medieval Europe respectively in the North (M. M. Postan, pp. 119-256) and South (R. S. Lopez, pp. 257-354), preceded by shorter surveys of trade and industry in Barbarian Europe till Roman times (V. Gordon Childe, pp. 1-32), under the Later Roman Empire in the West (F. W. Walbank, pp. 33-85), and in the Byzantine Empire (S. Runciman, pp. 86-118), and followed by more detailed descriptions of the Woollen Industry (E. Carus Wilson, pp. 355-429), Mining and Metallurgy (J. U. Nef, pp. 430-93)

and Building in Stone (G. P. Jones, pp. 494-518). Each section is supported by a full bibliography and four plates of charming illustrations demonstrate techniques of the building and cloth industries.

In divining eventual criticism the editorial preface is a masterpiece of anticipation. It is clear that where changes have been made they have been carefully considered and the choice has not always been easy. The relegation of urban history, for instance, conventionally associated with the study of medieval trade and industry, may fit equally well into the third volume, as the editors propose, but it is a pity that publication of this section, long expected and urgently needed, is to be further postponed. In the subdivision of its material the second volume seems less happy than the first. Specialised essays on 'short periods, small territories and circumscribed topics' may be more suited to the description of agrarian life and were apparently never part even of the original plan, but they do add substance and considerable value to Volume I and might have done the same for its successor. On the other hand, the conception of two central essays lends coherence to the main theme of this volume, and allows a long-range and long-term view of the growth of trade irrespective of narrow geographical limits and unrestricted by arbitrary division of time. Another instance where the cake, once eaten, cannot be had is in the principle which governs the Cambridge Histories. To prescribe a common lay-out and approach to chapters and to co-ordinate the individual contributions—however convenient for the student—would be to sacrifice the value of independent treatment and interpretation. As it happens, a general harmony—a belief in the survival of 'money economy', even in the immortality and glory of trade—pervades the whole. The protests of Dopsch against the 'catastrophic' effect of the Germanic invasions on Roman civilization are heard, while the claim of Pirenne that the Western European trade was choked to death by Moslem control of the Mediterranean is held to be not proven, and the medieval period is depicted against the background of a population rising and commerce expanding to about the middle of the 14th century. For particular differences of opinion, for repetition, and for lack of continuity no apology is offered: these are left to exercise the reader's critical faculty and emphasize the 'provisional' nature of both material and conclusions. The editors are clearly aware that it is a bold and responsible act to issue a history, which will rank as authoritative, in a subject which is full of controversy, and they are to be congratulated on presenting to the critical and watchful reader a view of current opinion in a form more easily comprehended than the detailed studies from which the evidence has been derived.

It is impossible to review each of the contributions in detail. Professor Gordon Childe draws a very readable sketch of economic life from what he calls the 'Stone Stage' to the Second Iron Age (La Tène). Professor Walbank describes the growing enterprise in the Roman Empire (c. A.D. 31-250), followed by the accepted course of decline in the 3rd century, the growth of self-sufficiency in the provinces, decrease of long-distance trade, depreciation of the coinage, and state control. But, while stressing the importance of the 'annona' and the reduced scale of 'money economy', he emphasizes the growth of 'adaeratio' which 'largely put an end to the experiment of taxation in kind' and the survival of local trade and 'irregular trade in luxuries from all parts of the Mediterranean' at the beginning of the medieval period. The Byzantine essay is something of an interlude. When 'with the loss of Syria and Egypt Byzantine economic history properly begins', it had nothing in common with the new communities of Western Europe. The city of Constantinople, pivot of a Far Eastern trade which fluctuated with the supply of western gold and the opening and closing of the trade routes, with her controlled coinage, taxation, and regulated economy, stands in strange contrast to the more primitive western

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standards. Perhaps the most significant contact with the West was the gradual surrender of Greek carrying trade to the growing cities of Italy.

The concentrated studies of single industries are in no less competent hands. The changing centres of the Woollen Industry—Flanders at her height in the 13th century, Italy in the 14th, and the gradual ascendancy of English cloth—the changing techniques both in organization and machinery, infant capitalism and class conflict, are displayed in some detail and with a precision which does full justice to this most important of medieval industries. Occupied with the problem why Western European civilization alone responded to the challenge of mineral wealth, Professor Nef unveils with enthusiasm and a sense of proprietary pride his communities of free miners (with their own body of customs in the 12th and 13th centuries), the great technical advances in the mining and treating of metals, and the unusual combination of scholarship and manual labour. The great increase in mining in the 12th and thirteenth centuries was followed by a slump in the 14th and early 15th which in turn led to improvements in all departments of the industry by such methods as the use of water power, blast furnaces, iron casting, and draining methods which enabled shafts to be sunk to the depth of four hundred to six hundred feet. The great boom of the early 16th century involved silver, copper, brass, cinnabar, alum, iron, steel and coal. Now the 'parts' of a mine, once held by working miners, were owned by financing groups and the mining communities replaced by associations of wage-earners. The increased stress on 'regalian' rights and the signs of large-scale enterprise, as in the great factories of the Fuggers, while of medieval making, bring us to the threshold of 'modern times'. If the section on Building is the shortest, it none the less gives as clear a description as available evidence permits. The medieval mason enjoyed considerable technical knowledge in design and methods of construction. Unskilled labour was readily available; skilled labour was assembled by contract or conscription. The control of both building and administration could be in a single hand, but from the 13th century tended to be divided. We are given proof of the existence of professional architects, and information on contracts and the forms of association of a mobile, wage-earning labour force.

The two general essays are necessarily handicapped by the editorial segregation of Northern and Southern Europe and by the absence of statistical evidence essential for such surveys. Dr Lopez is therefore confined chiefly to Italy and the rise of South German towns like Augsburg. He opens a convincing crack in Pirenne's theory by showing that whatever the quantity of trade, luxury articles from the East were generally available in Western Europe at a time when the Mediterranean is alleged to have been closed. What altered were the routes and the carriers. The long-distance traffic handled previously by the Levantine merchant was now funnelled through the border-peoples of the Empire, and Venice, instead of being the exception, becomes an example of the Italian middlemen operating in the 9th and 10th centuries. With expanding home markets and extended overseas operation by cities like Genoa and Pisa capitalist commerce reached its peak between the middle of the 13th and 14th centuries, and then entered on a great depression. We also have an insight into the methods of short-range commerce and local markets and into the widespread organization of sedentary firms which superseded the itinerant merchant and speculator of the formative period, and we are personally introduced to individual traders whose memory survives in the records. Professor Postan has perhaps the least enviable task. After an inspection of the commodities—essentials rather than luxuries—and the facilities and difficulties attending the North European traffic of goods, he subordinates the rest of the material to a pattern of an expanding and contracting economy, so organized as to unmask the rise and fall of population as the single basic

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cause. Untractable evidence is very skilfully deployed and the whole argument lucidly presented. In the 'expanding' sections he describes agrarian expansion, the rise of towns and the professional merchant, the expanding frontiers and changing carriers of industry and trade, and discusses the possible effects of the funds of gold available in the West, the Crusades and price movements on this increase in commercial activity. 'Contraction' is stipulated for the 14th and 15th centuries 'and more especially the second half of the former and the first half of the latter' (allowing short-term fluctuations to break the 'continuity of the falling trends'). We are given evidence and where possible statistics from the English Customs accounts, for the trade in grain, wool, wine, fish, salt, silver, iron, and cloth, to illustrate the slump, the story of 'regulated trade' and the fortunes of the Hansa towns to describe it, and the case for price movements and decreasing silver supplies to account for it. But this case is rejected in favour of that of falling population. The question will no doubt be asked whether the fragmentary and local statistics which survive should be allowed to establish a 'trend', and whether in a period so rich in local disturbances and variations one basic cause need or should be sought. Over a number of strong-points a line has been drawn which is logically tenable, but may yet fail to retrace the course of history. In the text and the preface Professor Postan does of course draw attention to the difficulty and scarcity of the evidence and the provisional nature of the conclusions. But there is a danger in this kind of presentation. What was 'a working hypothesis and a pretext for public debate' has graduated from a Report to the Ninth International Congress of the Historical Sciences (Paris, 1950) and the Economic History Review to the comparative orthodoxy of a text-book. 'So unless and until new evidence to the contrary is produced the commercial and industrial depression of the later Middle Ages must be accounted for by decline in numbers' (p. 216). It will be up to teachers of history to see that it does not pass automatically and unquestioned into examination doctrine, but receives the careful appreciation which it deserves. This is the due of any general theory and in no way detracts from its value. At a time when synthesis and opinion tend to be restricted to the secure privacy of lecture or common room it is a most commendable act to open a cherished theory to public inspection. The whole volume bridges a great gap in the information available to the English-reading public and makes a most welcome appearance.

E. O. BLAKE.

ANCIENT MONUMENTS IN NORTHERN IRELAND NOT IN STATE CHARGE. Introduction and drawings by E. M. JOPE, Lecturer in Archaeology in Queen's University. List of Monuments compiled by Officers of the Ancient Monuments branch of the Ministry of Finance (Works Division), for Northern Ireland. *Published by Her Majesty's Stationery Office, 1952, 16 plates, 19 text figures, map, 64 pp. text. Price, 2s.*

This small book is a valuable supplement to the earlier volume, *Ancient Monuments of Northern Ireland in State Charge*, 1940, now unfortunately out of print and practically unobtainable.

The ground covered ranges from the Mesolithic period down to the 19th century, and both the figures in the text and the eighteen plates of reproductions of good photographs add greatly to the value of the volume. Perhaps most useful of all is the annotated list of monuments arranged under the headings of the six counties of Ulster. The position of these sites is given, not by a map reading, but by such directions as 'about a mile to the north east of Cushendall'. This list will be invaluable to the traveller in the region, who will be well prepared when he has read the excellent though necessarily short, introduction to the archaeology of this district given by Mr Jope in his comprehensive essay.

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It is surprising to read that 'practically nothing is known about the Iron Age in Ireland apart from finds of fine metal work' and that great hill-top fortresses such as abound in Britain are hardly yet known as a class. Interesting accounts are given of Dark Ages round forts and linear earthworks, also of dwellings of the early Christian and Viking centuries, crannogs, raths and souterrains. In Anglo-Norman times the wooden towers and palisades which fortified the castle mounds were shipped from England ready made, and in 1429 a subsidy of £10 os. od. was granted to any landowner who would build a tower $20 \times 16 \times 40$ feet high to strengthen the defence of the English Pale. Surely an early housing subsidy. The Georgian architecture is suitably emphasized, and also the characteristic Civic buildings of the 18th and 19th centuries.

It is hard to agree with the author that 'timber circles have been revealed in England in those areas where stone is not plentiful', when we think of the Sanctuary near Avebury, or Woodhenge within two miles or so of Stonehenge.

As in other books, it would save the reader much time and temper if, when reference is made in the text to figures, the page as well as the number of the figure were given.

The book contains a map of the six counties and marks the ancient monuments, generally by number. We wish the names of the counties had been given. As a guide book should, this small volume leaves us longing to visit the region so engagingly described.

D.P.D.

THE AGRICULTURAL HISTORY REVIEW. Vol. 1, 1953.

Readers of *ANTIQUITY* will welcome the appearance of the *Agricultural History Review*, the Journal of the British Agricultural History Society. It is well printed and produced. It is hoped that it will appear at least twice a year. The first number is full of good things, the article by Sir James Scott Watson on 'Some Traditional Farming Beliefs in the Light of Modern Science' being of particular interest. The very widely held belief that one should plant with the waxing moon is shown to have no basis whatsoever. Other articles are on the Poll Tax and Census of Sheep in 1549; the Isle of Axholme before Vermuyden; Book reviews, and a Bibliography of recent books and articles on Agricultural History. We wish it every success. The subscription is one guinea a year, and the address is c/o Museum of English Rural Life, 7 Shinfield Rd., Reading.

H.W.E.

NEW MATERIALS ON THE LATER AEGEAN CHRONOLOGY AND HISTORY.

By STYLIANOS ALEXIOU. *Kretika Chronika*, 1952, pp. 9-41.

Dr Alexiou's new material is provided by his own excavation of two important tombs at Katsambas, the suburb east of Herakleion overlying part of the Minoan harbour town at the mouth of the Kairatos. Both were chamber tombs with dhromoi cut in the Kouskouras, the soft white rock of the district.

The chamber of the first tomb had a bench about 20 cm. high reserved in the rock along its South side. This contained 2 large palace amphorae, 3 smaller amphorae of the same type, a large bridge-spouted jug with 3 handles, a large one-handled jug with swelling body, 2 stemmed goblets, 2 double vases, the larger with birds perched on the rim, 2 square alabaster, 2 incense vases, the larger betraying traces of polychromy, 2 handleless cups, a bronze pin, pieces of a spear and of the handle of a sword, bronze handles (perhaps from wooden vessels) and rings of a chain. The decoration of the vases is the marine and floral type familiar in the L.M. II period, flowers, fruit, rows of leaves and argonauts combined with marine vegetation and the shapes are characteristic of the same period 1450-1425. This is L.M. IIA rather than L.M. IIB.

The main benefit from this characteristic tomb, as Alexiou points out, is that it fills out our rather meagre catalogue of L.M. II vases and in particular corrects the view of certain scholars who were inclined to regard the squat alabastron as only a Helladic, not a Minoan type.

The second tomb has more unusual contents, including a unique wooden larnax or coffin 1.30 m. long, .45 m. broad and .60 m. high approximately. The blue *kyanos* paint is compared by the excavator to the greyish blue colouring found by Perssen at Dendra and to the blue coating of the roof of the inner chamber of the Temple Tomb which Evans had suggested might be intended to produce an illusion of the blue sky.

On the west side of the chamber were discovered two Egyptian stone vases. One (PLATE IX), an alabaster amphora with painted foot and ring stand carved in one piece, bore the following inscription of Thutmose III (1504-1450 B.C.): Ntr nfr - Mn hpr rC - S'rC - Dhwtj mš nfr hprw - Dj Cnh dt: translated as: The good god - Men Kheper Re - son of the sun - Thutmose perfect in transformations - endowed with eternal life.

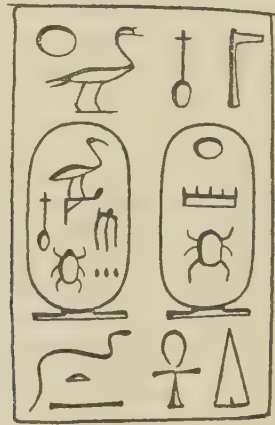


FIG. 1. INSCRIPTION ON ALABASTER VASE

The same inscription occurs on another alabaster vase, No. 18734 in the Cairo Museum. Other parallels to this shape occur, including one in alabaster found at Assur and pottery examples from Mycenae and Menidi.

The second Egyptian vessel is a diorite bowl of proto-dynastic type with flat rim and four unpierced semi-circular lugs. Another alabaster vase of the skyphos type, with handles added separately, may also probably be Egyptian (PLATE VIII). The first Minoan vase is a splendid beaked jug with painted argonauts and papyri designs of L.M. II type but also recalling the designs classified by Furumark as L.M. IIIa 1, divided into vertical panels by lines of plastic knobs with other plastic knobs round the neck, and a figure-of-eight shield rendered plastically under the spout. (This shape of vase is rare in pottery but often represented on Minoan seals showing demons making libations or bringing offerings, and there can be little doubt that it is a cult vessel.) Other objects in this tomb were 3 cups (1 with painted floral decoration) 2 blue tripod altars of stucco, 7 incense vases mostly blue but one polychrome with a design of leaves and stems, an almond-shaped

PLATE VIII



STONE AND POTTERY VASES FROM THE TWO CHAMBER TOMBS AT KATSAMBAS, CRETE

PLATE IX



EGYPTIAN ALABASTER VASE WITH INSCRIPTION OF THUTMOSE III; FROM A CHAMBER
TOMB AT KATSAMBAS, CRETE

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sard sealstone with a fine design of a lion attacking an ibex, a small necklace of gold beads, a silver pin, a small bowl and a knife, both of bronze.

Alexiou emphasizes the place and importance of this tomb. It is contemporary with the Temple Tomb, the tomb of the Double Axes, the tomb of the Mace-Bearer, and also I should add the Warrior Graves recently excavated by de Jong and Hood, and the unpublished tomb which I excavated near the Temple Tomb in 1940.

All these tombs are too rich to be dated after the great catastrophe, but must only just precede it. You may with Furumark call them L.M. IIIa 1, and they are I think parallel with his L.H. IIIa 1, but as they precede the catastrophe I should prefer to call them L.M. IIB, and their date must be, I think, about 1425 B.C. (with a few years' margin of error). Alexiou rightly stresses the fact that this tomb and its fellows show that Minoan Crete was still actively connected with Egypt throughout the L.M. II period, and that certain shapes such as the squat alabastron are not so exclusively Helladic as has been supposed. Both the blue wooden sarcophagus and the splendid beaked jug are unique objects hitherto unparalleled in Crete or on the Greek mainland. Alexiou demonstrates that Miss Kantor has underestimated the L.M. II cultural influences (probably because she was unfamiliar with some of the unpublished or inadequately published materials in the Herakleion Museum). I am indebted to Dr Alexiou for his kind permission to publish illustrations of this important material and also for supplying the photographs (PLATES VIII and IX).

R. W. HUTCHINSON.

CASTLES AND CHURCHES IN THE MIDDLE NILE REGION. By O. G. S. CRAWFORD: with a note on the inscriptions by M. F. LAMING MACADAM. *Published by the Sudan Antiquities Service, Occasional Paper no. 2, 1953; 41 pp., 30 plates, 12 maps and plans in text. 5s.**

Most of the castles and churches which are described in this monograph lie in the Middle Nile region between Abu Hamed and the Atbara. Some of the greater buildings are visible from the railway, but the region has been strangely neglected. Years ago notes on a few sites were published by H. C. Jackson, but his example has not been followed and it was this neglect which led Crawford to plan an expedition to the area.

The expedition was supported by the British Academy and the results have now appeared in a booklet which will be indispensable to all students of the history and antiquities of the Sudan. Fourteen ancient sites are included, some of the most important are new discoveries, and the plans, photographs and descriptions are, as one would expect, models of what such things should be: the beautiful photographs of churches in particular are precious records of buildings which look as if they were doomed to collapse before long. The Sudan Antiquities Service is to be congratulated on having secured such a report.

Excavation did not enter into the programme of the expedition and there is not one of the buildings which can be closely dated. The castles or forts are of very different periods: some of the walls may be Meroitic or older, others are comparatively recent, one of the forts at El Kab which is downstream of Abu Hamed may be as late as the 18th century. The three or four 'churches', on the other hand, form a singularly homogeneous group; they might all have been built by the same masons—small square or squarish buildings, one trapezoid in plan, the sides of the largest only about 40 feet long. Two of them are now in use as mosques and I have written the word *churches* between inverted commas because I am not sure that any one of the four in its present form was

* Kept in stock by Messrs Thornton, Booksellers, Broad St., Oxford.

built as a church. The materials, the burnt bricks, some with alpha and omega on them, the fragmentary inscriptions and much of the pottery found nearby, are obviously Christian. The construction of the three examined in detail—the mosque at Artul was neither planned nor fully described—*might* also be Christian: the bricks are laid very regularly in what is known here as the old English bond everywhere except on the roof level in Koro; the roof at the latter was carried on four piers, the sides being about 26 feet long; the roof at Usheir, the largest of the three, was carried on nine brick piers in three rows and six responds in the E. and W. walls (described, I think, in error as buttresses in the text). The construction was so good that some piers are still standing about 6 feet high though the roof has fallen. But I know of no churches in Nubia or farther south planned like these: the tiny recesses in one wall which are characteristic of all three make good Muslim mihrabs but would be useless as Christian apses, and we cannot exclude the possibility that the material is all re-used. There is not a scrap of decorative work on any of the three buildings.

The inscriptions are disappointing: fragments of seven inscribed tiles from Koro are published by Mr Laming Macadam, three in Greek, three in Coptic and one in a mixture of the two, but the name of the deceased and the date are missing on all with one doubtful exception on which the date A.H.305 may perhaps be conjectured. Others from a Christian graveyard at Wadi Dam et Tor are described as 'mostly too small for certain restoration'; they are shown on a plan in and near a group of what Crawford believes to have been Christian gubbass; the site of the adjacent village has been extensively dug over probably for sebakh.

Compared with the country round Meroë or the banks of the Nile in Dongola province this region is a poor one and it may be for this reason that so much has survived, how much we now know for the first time. Crawford tells us that the results of the expedition far exceeded his expectations. We can well believe it, and it is now up to the Sudan Antiquities Service to carry investigations a little further. A very little work should be sufficient to determine the dates of Usheir and Koro and other buildings.

The monograph has been printed in England by John Bellows Ltd., and the plans and photographs reproduced in the style familiar to readers of *ANTIQUITY*.

J. W. CROWFOOT.

BAAL IN THE RAS SHAMRA TEXTS. By ARVID S. KAPELRUD. *Copenhagen: G. E. C. Gad, 1952, pp. 156 octavo. Price, 21 shillings bound (18 shillings unbound).*

The poetic tablets from Ras Shamra (ancient Ugarit) on the coast of North Syria, of the 14th century B.C., are basic for reconstructing the remarkable development of literature in the East Mediterranean area during and after the Amarna Age.

Dr Kapelrud has analyzed, both comprehensively and in detail, the largest group of literary tablets from Ugarit: those dealing with Baal. His treatment is many-sided: philological, archaeological and comparative religious.

On p. 21, Dr Kapelrud calls attention to a Ugaritic seal cylinder on which supposedly a priest wears a bull-mask. The facts that Ugarit had a Minoan enclave and that an important god in the pantheon (Ktr-w-Hss, corresponding in function to Hephaistos) came from the Minoan sphere (called Caphtor), make it advisable to consider another interpretation; namely, that the man with the bull's head is in the Minotaur tradition. (Contrast the Sumero-Akkadian 'Bull of Heaven', which has a bull's body but a man's head).

The author and reviewer (see *Orientalia* 22, 1953, pp. 79-81) differ on certain phenomena as to whether they are seasonal or sabbatical. Specifically, Dr Kapelrud (in

the tradition of Frazer's *Golden Bough*) makes the death and revival of Baal a seasonal phenomenon. I, however, maintain that fertility during a normal year in Canaan is a round-the-year matter, with summer fruits and grapes ripening throughout the driest part of the year. Moreover, the Ugaritic texts themselves refer to the failing or death of either Baal or of his adversary Mot ('Death') in terms of sabbatical (i.e., seven-year) cycles. But Dr Kapelrud (p. 130) states that since 'eight' is mentioned with 'seven' in 1 Aqht : 42 ff. ('seven years may Baal fail, yea eight, the Rider of Clouds; let there be no dew, no rain . . .'), we are dealing with a round number meaning 'several' or 'many'. Even if Dr Kapelrud were right, this would take the phenomenon of drought and famine, due to the failing of Baal, out of the scheme of seasonal pattern, for anything that persists for 'several' or 'many' years is not seasonal. However, it is interesting to note that the climactic use of 'eight' actually fits into the sabbatical scheme. After seven sabbatical cycles comes the jubilee year in which the land lies fallow. In other words, if the seventh sabbatical cycle is one of distress, the catastrophe is prolonged by the following (= 'eighth') jubilee year (Leviticus 25 : 11). That this 'eight' is not a loosely used cliché is evident from the fact that 'seven days' are never climaxed in Ugaritic by an eighth day, because the week cannot be stretched out beyond seven days, whereas 'seven years' can be climaxed by the 'eighth' corresponding to the reality of the fiftieth year attached to the last sabbatical cycle of each jubilee cycle.

The conventional view that the sabbatical and jubilee cycles are late and artificial legislation, is unjustified. Orthodox Jewry in Israel still resorts to legal fictions to circumvent the duty of letting the land lie fallow in the seventh year. Josephus refers to the sabbatical year as an institution that seriously affected national life and defence. The Book of Jubilees, while quite likely confronting us with an innovation insofar as it uses jubilee and sabbatical cycles for establishing an *Anno Mundi* chronology, must be using an ancient system of grouping years, because of Old Testament evidence in the preëxilic Prophets independently confirming the antiquity of the Pentateuchal legislation. In Zedekiah's reign, Jeremiah (34 : 8 ff.) reports the attempt to enforce the freeing of Hebrew slaves in the sabbatical year. There was no question as to when that 'seventh' year fell for the system had survived as a means of reckoning time, though the laws connected with it had fallen into desuetude because of the material sacrifices they entailed on the part of the upper classes. The event reflects a vestigial, not a nascent, institution.

Most significant, however, is Isaiah 37 : 30 on the occasion of Sennacherib's invasion of Judah in 701 B.C. : 'And this shall be a sign for you : This year, eat *sāfi^{ah}* ; and in the second year (eat) *šāhīs*,* and in the third year, plant and reap ; and plant vineyards and eat their fruit'. *Sāfi^{ah}* is what grows of itself when the land is left fallow ; according to Leviticus 25 : 5, 11 it was not to be eaten. *Šāhīs* is the technical term for spontaneous growth in the second consecutive year that the land lay fallow. The existence of so specialized a word is explicable only by the actuality of the institution whereby the land was left fallow in the forty-ninth and fiftieth years. What happened in 701 is clear : Rabshakeh's speech (II Kings 18=Isaiah 36) shows that Assyrian military intelligence was well informed on conditions and institutions in Judah. Isaiah 37 : 30 informs us that the invasion of 701 was timed so as to strike Judah at its most vulnerable time : when the forty-ninth and fiftieth years created a food crisis. (This makes Rabshakeh's threat of famine in II Kings 18 : 27 the more pointed.) Isaiah therefore (1) predicted that a sufficient spontaneous growth would be the sign of God's providence and (2) granted a dispensation allowing the people to eat of that growth even though this was ordinarily forbidden (Leviticus 25 : 5, 11).

* Variant (with metathesis) : *sāhīs* (II Kings 19 : 29).

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The pre-Mosaic Ugaritic evidence shows that the Hebrews inherited the sabbatical cycle and jubilee (in broad outline, though not necessarily in specific detail) from the Canaanite past.

One of the elements that went into the jubilee complex (to wit, the return of land to the original owners) seems to be of Hurrian derivation; for the real-estate transactions in the Hurrian community at Nuzu (near Kirkuk, Iraq, in the 15th and 14th centuries B.C.) are characterized by obvious circumvention of the inviolability of land. In *Revue Biblique* 44, 1935, pp. 38-41, I associated the Nuzu releases with the sabbatical and jubilee releases. The comparison (however much the details remain to be worked out) still stands as an organic parallel. Since Palestine was part of what the Egyptians called Hurru-land, Hurrian influence on Canaanite and Hebrew institutions during the 'Hurrian Millennium' (i.e., the second millennium, B.C.) fits into the established framework of history.

The non-seasonal factors in ancient calendars must take their place beside the seasonal ones. The Greek Olympiad transcended the seasons (falling once every four years) and served as a means of reckoning chronology. So did the sabbatical and jubilee cycles in Canaan.

My difference of opinion with Dr Kapelrud on this issue should not conceal my high regard for his scholarship in general and for the book under review in particular.

CYRUS H. GORDON.

ARAMAIC DOCUMENTS OF THE 5TH CENTURY B.C. By G. R. DRIVER, M.A., F.B.A. *Oxford University Press*, 1954, 59 pp., 24 plates. 84s. net.

This book is an interesting yard-stick by which to measure the progress made in the study of Semitic palaeography, philology, and lexicography during the last half-century. Between 1906 and 1911 we were studying the famous Aramaic papyri from Elephantiné with the help of the editions provided by Sayce-Cowley and by Sachau. Now, in this magnificent edition of a group of Aramaic documents on leather, coming from Egypt, belonging to the same period of Persian rule in Egypt, and mentioning the same Persian satrap of Egypt whose name occurs in the Elephantiné papyri, it is possible to estimate the immense advances which have been made in comparative philology since the publications above mentioned.

In 1933 an unnamed dealer brought to the late Professor Borchardt a bundle of documents written on leather; the dealer either did not know, or would not reveal, the place where they were found. From photographs the late Prof. Mittwoch recognized that they dealt with the same period of Persian history as the Elephantiné papyri; Dr Borchardt accordingly bought them, and after his death they came into the possession of the Bodleian Library, where they were carefully arranged and mounted between glass. Now, at long length, Professor G. R. Driver's patient and learned labour has made them available to scholars. This edition contains an introduction discussing the problems of their date and place of origin, and giving an account of the persons mentioned and the historical setting of the documents. Then follows an accurate transcription and translation of the thirteen more or less complete letters with very full and valuable textual and philological notes. These will furnish a quarry for industrious scholars for many a year to come. In an appendix Professor Driver has collected all the evidence relating to Arsames from Neo-Babylonian, Greek, and other Aramaic sources, and has completed his editorial work with an excellent glossary. Lastly come the 23 photographic reproductions, whose perfect clearness will delight the heart and aid the sight of the palaeographer.

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Most of the letters were written from Babylon or Susa by Arsames, Persian satrap of Egypt, during his absence from Egypt between 411 and 408 B.C. They are addressed to various Persian officials in Egypt, and it is in the diplomatic bag of Nehtihur, one of these officials, that this correspondence has probably been preserved. Plates xxiii a and b are photographs of the bag in question.

By an irony of history the letters show that the Persian empire found Egypt as difficult a problem as Britain does to-day, even down to the detail of the kidnapping of her soldiers. It was during this same absence of Arsames that the destruction of the Jewish temple at Yeb was carried out by insurgent Egyptians, perhaps with the connivance of the local Persian governor—an episode which forms the subject of some of the letters and documents comprised in the Elephantine papyri. The letters give a valuable picture of the details of provincial government under the Persian empire, and all the day to day problems that come up for settlement. Document VI is particularly interesting: it is a passport or letter of credence issued to Nehtihur for his journey from the capital back to Egypt. It gives instructions to the local governors at the various stages of his route to supply him and his suite with everything necessary for the journey. Amusingly, to preclude dallying at any stage, governors are instructed that no provision is to be made for more than one day's stay at any place.

It is tempting to linger on many interesting philological points that arise, but this would swell this review beyond due limits. One thing, however, must be said. The immense progress in the field of Semitic philology made during the last half-century as illustrated in this book, is in no small measure due to Professor Driver's own contribution to these studies. Of the various major works which have appeared under his name in recent years, none will prove a more valuable addition to the field of Semitic scholarship than this magnificent *editio princeps* of these important Aramaic documents. The Oxford University Press, also, must be congratulated on the excellence and accuracy of the production.

S. H. HOOKE.

THE FARWELL COLLECTION. By FRANKLIN P. JOHNSON (Monographs on Archaeology and Fine Arts sponsored by the Archaeological Institute of America and the College Art Association of America,* VI). *Cambridge, Mass.*, 1953. pp. VIII+76, 90 figs. on 20 pls. \$4.15.

Towards the end of the war Captain Farwell, an officer of the United States army, was stationed in Apulia and employed his leisure in excavating some twenty graves at Ortona, the ancient Herdonia. The contents were shipped to America, where they were studied by Franklin P. Johnson at Chicago, and are now published with scrupulous pains. The ethics of such an excavation are debatable, and Johnson does not shirk this issue. One can only say that the grave-groups have been treated with at least no less care than those dug at Ortona by Italian antiquarians and archaeologists fifty years and more ago, and that as a result of Captain Farwell's zeal and Professor Johnson's scholarship the biggest and best-defined group of Daunian pottery is here adequately presented.

Daunian is one of the most easily recognized styles of native Italian pottery of the Iron Age, and the cemeteries of Apulia have been abundant; few big museums are without some pieces. And it has a certain attractiveness, with its grotesque forms and bright colours. But its study has not advanced very far, largely because of the lack of detailed information about conditions of excavation—grave-groups, association with imported vases. The present work, with its careful descriptions, good illustrations, and citation of

* The address of this body, not given in the book, is Andover Hall, Francis Avenue, Cambridge 38, Mass., U.S.A. ED.

published parallels, makes a good beginning. The 208 vases in the collection (half in grave-groups, half now without associations) fall into three groups: hand-made vases decorated in matt paint in one or two colours; wheel-made vases with lustrous paint; and a small group of Greek vases, made in one or more of the Italiote cities. The latter fall within the 4th century, most of them in its first half, and provide a date for the graves from which they come. The wheel-made vases show Greek influence in shape at many points. This influence is mainly Attic, of the 5th century and first half of the 4th. A number of East Greek (Rhodian and other) parallels are cited; this is the more remarkable in view of our scanty knowledge of East Greek pottery of the 5th and 4th centuries. The Rhodian element in the formation of the native Apulian styles has been observed before (Mayer, *Apulien*, 267 ff.) but is here given more precise, and later, dates. This bears on the traditions of Rhodian, Cnidian, and other East Greek settlement on the Adriatic coasts of Italy (see R. L. Beaumont, *JHS* 1936, 172 f.; cf. 192 f.); one of the places to which a Rhodian origin was ascribed is Elpiai or Salpi, which lay on the coast at almost the nearest point to Ortona. Corinthian influence is rare (p. 64), and some of the Corinthian parallels suggested are doubtful. This is primarily a chronological point; there is little Corinthian pottery of a date later than 550 in this part of Italy; as Johnson says, 'it is possible that, when Corinthian pottery later than 550 is more fully studied, its influence may be definitely traceable in other pieces'. Likenesses with earlier Greek pottery seem to be accidental. Again, it is relevant that some communities on the Adriatic coast, in the area of Monte Gargano, were still using Bronze Age techniques in the 6th century, as is shown by the occurrence of Greek sherds of that date (see E. Baumgartel, *BSR* xxi, 1953, 1 ff.). The potter's wheel apparently reached the workshops where the Ortona vases were made in the 5th century, or perhaps the late 6th; hand-made vases of course continued to be made, but there are a number of graves with only hand-made vases, which are, it seems, earlier. How much earlier, one cannot say, but they are unlikely to be earlier than the 6th century. A few of the hand-made vases show the influence of Greek shapes of the 6th century.

The most interesting vases are hand-made, and early. Johnson deals very thoroughly with technique and shape. It is a pity that he does not say more about their decoration; for instance, the little silhouette figures of highly stylized geometric men and birds; and the bowls with high handles which bear a schematic resemblance of the human figure. The wide range of formal parallels for these, among which the elaborate modelled vases of Bronze Age Cyprus are among the most striking, might suggest something about the use of these bowls; for similar forms elsewhere appear to be funerary.

Interest in northern Apulia has been lively lately. It is to be hoped that recent and forthcoming work on this area, a rich area which lay open to contacts both with the Greeks from the south, and with Illyria across the Adriatic, may throw more light on the early cultures of Italy, particularly in the Early Iron age.

T. J. DUNBABIN.

A COMPARATIVE GRAMMAR OF THE HITTITE LANGUAGE; Revised Edition: *Volume I*, by E. H. STURTEVANT and E. A. HAHN; pp. xx+199; *William Dwight Whitney Linguistic Series*; Yale University Press, 1951. Price \$5.00.

This volume is essentially the late Professor Sturtevant's comparative grammar of 1933 revised with the help of Professor Hahn. For the present, however, it must be considered the most authoritative work on the comparative linguistics of (Cuneiform) Hittite, the official language of the original Hittite state of Anatolia c. 1600-1200 B.C. Most of the equations and etymologies proposed by the authors are accepted by other

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specialists in their subject, though minor points of controversy are numerous, and many questions will probably never be settled definitely owing to the inexact representation of the original spoken Hittite forms in the cuneiform texts. The paradigms on pp. 92-101, 114-5, 149-65 are valuable as descriptive grammar.

The serious defect of the book is that it treats as proved two conclusions reached by Sturtevant which have not found general acceptance: the 'Indo-Hittite hypothesis', according to which Hittite is not derived from the immediate ancestor of the languages generally recognized as Indo-European (see p. 9), and the view that the ultimate ancestor of Hittite and those languages had four consonantal phonemes ('laryngeals') which the latter lost (see pp. 29, 47-55). In the reviewer's opinion there is no proof that all other dialects of Indo-European ('Proto-Indo-Hittite') remained in contact after losing contact with Proto-Anatolian, the dialect from which Hittite evolved. (See now J. Friedrich, *Journal of the American Oriental Society* 73, 107-9.) As for the 'laryngeals', equations listed on pp. 47-51 indicate that Indo-European had at least one phoneme which was lost in most derivative languages but survived in Hittite as a phoneme written with the cuneiform signs used for writing the velar fricative of Akkadian; and there is a good case for postulating two such phonemes, the authors' α and γ . However, the arguments adduced for the existence of their h and $'$ are not convincing. (See *inter alia* the reviewer's observations in *Transactions of the Philological Society* 1951). The grammar would have been both more scholarly and more useful to the non-specialist reader if the authors had first set out the equations and etymological explanations which are generally accepted, and then presented their arguments for their own theories and the revised phonology which they propose for the ultimate ancestral language. They should at least have referred to the extensive recent literature criticizing their views.

The *Introduction* gives a useful indication of the extent of present knowledge of the pre-hellenic languages of Asia Minor. However, the 'family-tree' on p. 9 is speculative, Sommer's demonstration that pre-Hittite was brought into Central Anatolia from the east is not conclusive, and it seems unlikely that Hittite was reduced to writing as early as c. 1950 B.C. (p. 18). The suggestion that the Hittites learnt the use of cuneiform from a pre-Hurrian people of Syria deserves serious consideration. Finally, the authors have not sufficiently emphasized that the extant Hittite material includes groups of texts composed over a period of at least four centuries, and perhaps others subject to local dialect influences, and that the linguistic strata in it have not yet been clearly distinguished.

There is little to criticize in the chapter on phonology except the sections concerned with the 'laryngeals'. More consideration should perhaps have been given to the possibility of secondary changes, especially contractions, in Hittite. The idea that an initial voiced labio-velar of Indo-European gave Hittite w - has wisely been abandoned (p. 57).

To turn to morphology, the rejection of Güterbock's demonstration that Hittite had retained certain vocatives seems unjustified (p. 12, n. 4). The distribution of $-\bar{e}-$ and $-u-$ in second singular pronominal paradigms can be explained without setting up different paradigms for Proto-Anatolian and 'Proto-Indo-European' (p. 103). The explanation of the indicative paradigms of the *hi*-conjugation on pp. 132 and 142-4 is not completely satisfactory. It is unlikely that both these tenses developed from the perfect. The present is probably derived in part from the category represented by the Greek present in $-w$.

There are very few misprints in the grammar. It would have gained as a work of reference by greater use of tabulation and of heavy type in headings. The general use of syllabic transcription is welcome. The consistent employment of w and y , instead of

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u and i, in reconstructions, though logical, has a bizarre effect. A few references for the forms listed in the paradigms would have been worth the cost of printing.

Volume II by Professor Hahn should be a valuable contribution to the literature of Hittite studies if, as one expects, it is to be a primarily descriptive treatment of Hittite syntax.

R. A. CROSSLAND.

ETCHED BEADS IN INDIA. *Deccan College Monograph Series*, No. 4. By M. G. DIKSHIT. pp. viii+41, plates 19. Poona, 1949. Rs. 10.

NOTES ON SOME INDIAN AMULETS. By M. G. DIKSHIT. *Prince of Wales Museum Bulletin*, No. 2, 1953, 86-99, xv pl., Bombay.

SOME BEADS FROM KONDAPUR. *Hyderabad Archaeological Series*, No. 16. By M. G. DIKSHIT. pp. viii+32, plates 8, Hyderabad, 1952. N.P.

Dr Dikshit having collected a great deal of information about ancient Indian beads was, like many another, faced with the difficulty of getting his material published; hence these scattered monographs. By far the most important is that on etched beads, which covers the whole of India at all periods, and provides a corpus illustrating the whole range of known patterns, with notes on their areas of distribution. Dikshit's pattern 13, a number of loops etched round the margin of a circular flat bead, seems to be fairly common in the North and this would discount the probability of the great antiquity claimed for a bead of this type by Beck (Beads from Taxila, p. 3 and Pl. ii, 31). Etched beads appear in small numbers at Harappa Culture sites, but so far their continuity down to the historic period remains untraced.

Dr Dikshit's amulets are all animal beads, and he gives a clear statement of the amuletic value of all these lucky charms which have an enduring history throughout the world from early in the 3rd millennium down to the present day. He omits however the chrysalis beads of lapis common in the North West and also non-animal charms such as the triratna.

Kondapur in Hyderabad State is a site excavated by the State Archaeological Dept. in 1940, and though no very reliable results can be claimed, the occupation revealed is of the Sātavāhana period. Fortunately the site of Brahmapuri, Kolhapur, provides a well stratified check for the material of this period, of which Dr Dikshit makes full use in his monograph on Kondapur beads. The author produces parallel examples from sites all over India for most of the beads dealt with; it is doubtful however whether these very durable objects, though in some cases datable, can ever themselves have much dating value.

D. H. GORDON.